IPCC Working Group I Fourth Assessment Report

Expert and Government Review Comments on the Second-Order Draft

Chapter 4

The following compilation of review comments and author responses is supplied by the Working Group I Technical Support Unit as a record of the process used to prepare the Working Group I report. These comments and responses are not to be edited and/ or re-distributed in part or in full to others.

Please note that under IPCC procedures authors are required to take account of all substantive review comments in both review rounds. Thus responses to individual comments may be influenced by comments from other reviewers.

Batch AB (15 June 2006)

No.	Batch	Page:line From	Page:line To	Comment	Notes
4-1	A	0:0	0:0	Please ensure that the final version of the chapter avoids referring to programs or to research needs issues. The last submitted version did not do so, as has been discussed with the CLAs. [Susan Solomon (co-chair WG1) (Reviewer's comment ID #: 246-1)]	Accepted. Text modified
4-2	A	0:0	1:	It is dishonest to use the results of Zwally et al (2005) for Antarctica, because they show a shrinking, and suppress their results for Greenland, because they show an increase, of which you evidently disapprove. [VINCENT GRAY (Reviewer's comment ID #: 88-555)]	Rejected. Results of Zwally et al. are included in Figure 4.6.2, and discussed in 4.6.2.2 (i)
4-3	A	0:0		Need to ensure that the snow section in Ch3 3.3.2.3 links in to this chapter [Govt. of Australia (Reviewer's comment ID #: 2001-258)]	NOTED. [the LA for snow in chapter 4 is also a CA for chapter 3 3.3.2.3]
4-4	A	0:0		This chapter is of good length and has a succinct Executive Summary. One weakness noted by our review is that while an indication of the errors is given in Fig 4.6.2, the chapter itself lacks comment on sources of error in the data and information on uncertainties. [Govt. of Australia (Reviewer's comment ID #: 2001-259)]	Accepted. Text modified to accommodate consistent treatment of uncertainties.
4-5	A	0:0		I find chapter 4 interesting, well written and consistent with the other observation chapters of the SOD . The text is improved in clarity, compared with the FOD. [Roxana Bojariu (Reviewer's comment ID #: 24-1)]	Noted.
4-6	A	0:0		Ok [Tiziano Colombo (Reviewer's comment ID #: 46-12)]	Noted.
4-7	A	0:0		The chapter uses three versions of "per year": y-1; a-1; and yr-1. Consistency is also needed across all chapters. [Chris Folland (Reviewer's comment ID #: 71-70)]	Accepted. Text modified. Throughout the report yr^{-1} is used.
4-8	A	0:0		COMMENT: This is very nice review of the subject! [William Hare (Reviewer's comment ID #: 99-22)]	Noted.
4-9	A	0:0		COMMENT: A question for the overall conclusions in relation to trends in deglaciation (glaciers, small ice caps, Greenland and Antarctica is whether or not to emphasized that the increasing loss of ice is linked to increased warming (and the significance of the apparent accelerating ice loss). At present the conclusions indicate an increasing rate of loss of ice and a general link to warming (Exec summary). [William Hare (Reviewer's comment ID #: 99-23)]	Attribution is not an issue for Chapter 4. There is a section on "Consistency aross observations" in Chapter 3.
4-10	A	0:0		The structure of this chapter is good and it focuses on the most current findings about ice sheets and outlet glaciers. The big issue is of course the rapid changes in marginal ice elevation and increased ice flow velocities of the outlet glaciers in West Antarctica and Greenland. The part of the section handling this issue is well balanced and well written.	Noted. No specific action requested. Text has been modified to reflect the importance of glaciers.

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				The parts concerning glaciers and permafrost are lacking the spectacular recent findings on the ice sheets, and are thus less well represented. I think this is all right with the permafrost where many projects are starting up now and results will be much more comprehensive to the fifth assessment report. But glaciers should not be so much in the shadow as they are at present. Though no spectacular science news have reached us during the last 5 years, they are still the most direct reminder of a changing climate. They influence temporal water discharge variations in mountain rivers by smoothen the discharge over the year, they influence the distribution of freshwater in the Arctic, they influence sediment transport in rivers, they have local effects on climate etc, etc. They are also the supreme study objects for detailed mass balance calculations and energy balance measurements, knowledge of importance both for the climate monitoring and to understand processes acting on the ice sheets. And, finally, they will vanish first as an effect of global warming. [Per Holmund (Reviewer's comment ID #: 108-2)]	
4-11	A	0:0		Ch 4 - The fact that none of the Lead Authors' expertise is in sea ice observations appears to have left most of the responsibility to the contributing authors, with some sections/sub-sections lacking the character of the others. For example, the contributing author of the first two sub-sections of Section 4.4.2 "Sea ice extent and concentration" appears to have ignored essentially all of my previous comments (see specific comments below). This is in contrast to the authors of other sections (e.g., Section 4.6.2 "Mass balance of the ice ie sheets and ice shelves"), who have properly taken in my previous comments. [Ola M. Johannessen (Reviewer's comment ID #: 119-1)]	Noted. 1. The reviewer is wrong concerning the expertise of the LAs. 2. The IPCC is an assessment and not a review.
4-12	A	0:0		The chapter reads very well indeed, a big improvement from the FOD - I can find almost no problems. Very nice, well done. [James Renwick (Reviewer's comment ID #: 211-1)]	Noted.
4-13	A	0:0		General: was there a specific decision made to limit discussion of NAM/SAM influence on sea ice and Arctic/Antarctic temperature change to other chapters? [David Rind (Reviewer's comment ID #: 214-30)]	The influence of NAM/SAM on sea ice is discussed in 4.4.4.2. The influence of NAM/SAM on Arctic/Antarctic temperatures is a matter of Chapter 3.
4-14	A	0:0		General comment on chapter 4. Comments have been limited largely to the sections on permafrost and frozen ground. There appears to be a lot of information provided that is of a more general nature and is largely related to impacts of permafrost thaw or decrease in extent of frozen ground. This discussion would be more appropriate for WG2 (eg Polar chapter). The focus of this chapter would appear to be the indicators of climate change and trends in cryospheric conditions (observed changes in climate system). As mentioned in the specific comments provided, there are a number of places where the text could be reduced (move discussion to WG2) and this would allow expansion of discussion on	Noted. Text has been modified. Impact issues have been communicated to WG 2.

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				trends in permafrost conditions and also inclusion of additional references. Suggestions have been made in specific comments where additional and more recent references should be added. While it is good to cite papers that present an overview, it is also important to cite the original references as this gives a better idea of the body of knowledge on which the various statements are based and would greatly benefit the reader. [Sharon Smith (Reviewer's comment ID #: 244-57)]	
4-15	A	0:0		We have noticed that the analysis of changes of glaciers and ice sheets are to some extent vaguely handled. Of course the effects of these changes will be more relevant for WG II, but there could be some linking to such discussion. [Govt. of Sweden (Reviewer's comment ID #: 2020-18)]	Rejected. Chapter 4 is limited to observations and their analysis. Impacts are dealt with in WG2.
4-16	A	0:0		Unfortunately, the reference to Tamisiea et al., 2006, must be removed. I have indicated this to both Anny Cazenave at Fall AGU and to Jürgen Willebrand via email in February. [Mark Tamisiea (Reviewer's comment ID #: 262-1)]	Does not apply to Chaper 4 but to Chapter 5. Ch. 5 Response: Accepted, reference removed.
4-17	A	0:0		There should be an emphasis on temperatures and links to Chapter 3 discussions, just as there is for accumulation rates. This interannual variability explains why the trends considered over different intervals are different. Seasonality of the trends are another important factor in apparent differences between studies – for example, the Thompson and Solomon (2002) cooling is limited to summer and autumn, and is not an annual mean trend. The current text overlooks these factors and implies that all the trends quoted are annual mean, which is incorrect. There is further confusion in the summary of the new Chapman and Walsh study. [Govt. of United States of America (Reviewer's comment ID #: 2023-261)]	Rejected. See No. 4-3 above. 1. Chapter 4 is limited to observations of the cryosphere. Consistency across observations is dealt with in Chapter 3. 2. Discussion on the trends always states, which trend is meant. "Trend of the annual mean" exactly states what trend is discussed: the trend in a time series of annual mean values (similarly for the summer minimum).
4-18	A	0:0		A possible deficiency of this chapter is that it evidently fails to present the information on Antarctica (and perhaps Greenland) in the format required to ensure that the Chapter 10 authors use its results. Surely the Chapter 4 authors recognize the disconnect between this chapter's finding that the major ice sheets are already making a net contribution, and the Chapter 10's finding that they will not do so until after the year 2100. The discrepancy may result from different schools of thought being represented in the different chapters, but the authors of Chapter 4 should re-examine their chapter to evaluate whether an alternative specification of their own findings might make it easier for the results to feed into Chapter 10. For example, what is the uncertainty range for the historical sensitivity of Antarctic ice sheet (mm/yr) to temperature changes? Such a parameterization could be passed directly to the Chapter 10 assessment. Chapter 4's failure to provide a climate sensitivity parameter leaves Chapter 10 with little choice other than to use pre-existing	Noted and disagreed. Discrepancies clarified with Chapter 10. Rejected. Ch. 4 is not supposed to deliver a sensitivity parameter

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				models, which may or may not include all of the insights embodies in Chapter 4. [Govt. of United States of America (Reviewer's comment ID #: 2023-262)]	
4-19	A	0:0		Suggest including more discussion of better characterized embedded shorter period trends to balance discussion of trends computed over long periods. Readers will concentrate on the long-term trends which, when considerable shorter-term variability is present, will be strong functions of the conditions at the start and end of the record and not indicative of important changes on shorter time scales. This comment reflects some of the specific comments received on this chapter concerning the statistical analysis to extract trends from a record containing strong fluctuations at various time scales. [Govt. of United States of America (Reviewer's comment ID #: 2023-263)]	Rejected. Shorter period trends are even more subject to the conditions at the beginning and end of the period considered.
4-20	A	0:0		Use of "likely" and other terms reflecting certainty or confidence of a statement in the chapter are inconsistently applied. There are numerous instances where formal terms of certainty or confidence defined elsewhere in the assessment, in particular, the Technical Summary, have been used to qualify a statement in an informal and inappropriate sense for the assessment. Recommend that the authors conduct a global search and evaluation for consistent use of these terms throughout the volume. These terms include, but are not limited to: "likely", "caused", "confidence", "attribution". [Govt. of United States of America (Reviewer's comment ID #: 2023-264)]	Accepted. Text clarified.
4-21	A	0:0		Chapters 4 is supposed to focus on results from observations, but frequently went beyond the summary of recent observations in the literature into explanations and discussions of attribution. For instance, the section on "Consequences" in Chapter 4 seems wholly out of place. Such discussions of attribution have extended the length of the observation chapters and lead to an uneven presentation. Recommend removing these discussions, or if appropriate, moving them to Chapter 9. Also strongly recommend a substantial shortening of Chapters 3, 4, and 5 in order to make them more even in presentation, as well as more focused, and improve the ease of reading. [Govt. of United States of America (Reviewer's comment ID #: 2023-265)]	Consequences removed. Noted. No specific suggestion.
4-22	A	0:0		Was there a specific decision made to limit discussion of NAM/SAM influence on sea ice and Arctic/Antarctic temperature change to other chapters? [Govt. of United States of America (Reviewer's comment ID #: 2023-266)]	See 4-13.
4-23	A	0:0		Provide observation-based support (or explain the lack of support) for a paragraph in Chapter 10 (page 60, especially lines 20-23). The statement says, in effect, that the major ice shelves cannot have significant surface melt unless the average summer temperature is above freezing. Given the importance of surface melting to Chapter 10's analysis, it would be helpful to discuss (a) whether periods less than an entire summer can induce significant melting and (b) whether interannual variability is enough for significant melting to occur during some years while the mean summer average remains below	Discussed with Ch. 10 to rephrase paragraph.

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				freezing. [Govt. of United States of America (Reviewer's comment ID #: 2023-267)]	
4-24	A	0:0		I found this chapter to be an interesting read - it is well written and tells a compelling story. I noticed that units are not quite consisent throughout the chapter (some parts use volume units to express changes in the cryosphere, while other parts use mass units - my personal preference is for the former). Also, it is evident that a number of the data sets that are used have not been updated. [Francis Zwiers (Reviewer's comment ID #: 305-1)]	Noted. Units adjusted. Time series updated where possible.
4-25	A	0:0		The reader is often not told how to interpret the uncertainty ranges that are given in the ES and elsewhere. [Francis Zwiers (Reviewer's comment ID #: 305-2)]	Accepted. Uncertainty ranges clarified.
4-26	A	1:11	1:11	Write Jon (not Jan) - Ove Hagen [Wilfried Haeberli (Reviewer's comment ID #: 94-2)]	Accepted. Text modified.
4-27	A	1:11	1:11	Spelling mistake for "Jan-Ove Hagen" --> Correct is: "Jon Ove Hagen" [Ketil Isaksen (Reviewer's comment ID #: 115-1)]	Accepted. Text modofied.
4-28	A	2:0		Should there be use of the IPCC calibrated language in the ES? [Francis Zwiers (Reviewer's comment ID #: 305-3)]	Accepted. Text adjusted.
4-29	A	2:1	2:2	"0.4 m/year on the Tibetan Plateau" is a wrong message appeared here. In page 4-31, paragraph 3, line 4 to 5, the authors described as: Over the Tibetan Plateau, the basal thawing rate of about 0.01 to 0.02 m per year was observed since 1960s. So here should be 0.02 m/year. [Govt. of China (Reviewer's comment ID #: 2006-46)]	Accepted. Text modified. Should be 0.04 for Alaska and 0.02 for Tibetan Plateau.
4-30	A	2:3	2:3	Please insert …, claciers and ice caps,…. [Govt. of Germany (Reviewer's comment ID #: 2011-7)]	Accepted. Text modified.
4-31	A	2:3	2:3	Add "glaciers and ice caps" after sea ice [Wilfried Haeberli (Reviewer's comment ID #: 94-3)]	Accepted. Text modified.
4-32	A	2:4	2:4	Instead of frozen ground it would be better to say permafrost and seasonally frozen ground (should probably change this in title of chapter also) as this is acceptable terminology in other international programs and reports. [Sharon Smith (Reviewer's comment ID #: 244-1)]	Rejected. Chapter title aggreed upon in the IPCC plenary; not subject to changes. Use of terminology clearly shown at beginning of frozen ground section.
4-33	A	2:4	2:5	There are also links with gas fluxes including the carbon cycle [Sharon Smith (Reviewer's comment ID #: 244-2)]	Accepted. Text modified.
4-34	A	2:7	:7	Remove the comma [Richard Soulen (Reviewer's comment ID #: 248-41)]	Accepted. Text modified.
4-35	A	2:8	2:8	Cross refer to CH6.	Accepted. Text modified.

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				[Chris Folland (Reviewer's comment ID #: 71-71)]	
4-36	A	2:9	2:9	Replace "correlated with" with "influenced by" [VINCENT GRAY (Reviewer's comment ID #: 88-513)]	Rejected.
4-37	A	2:9	2:9	Insert after "century . "or so." [VINCENT GRAY (Reviewer's comment ID #: 88-514)]	Rejected.
4-38	A	2:10	2:10	Delete"," after "65°N". At the end of this sentence,add the following statement " , and the region of tropical–subtriopical where glaciers are sensitive to climatic changes. " [Govt. of China (Reviewer's comment ID #: 2006-42)]	Rejected. Inconsistent with chapter findings.
4-39	A	2:10	2:10	Delete "by about twice the global average" This claim is disputable. It is based on land-based weather statioins that are likely influenced by local heating [VINCENT GRAY (Reviewer's comment ID #: 88-515)]	Rejected.
4-40	A	2:12	:1	You should announce from the start what is meant by your confidence limits. If they are only one standard deviation they must all be doubled to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-529)]	Accepted. Uncertainty estimates clarified.
4-41	A	2:14	2:14	English must be smoothed [Ketil Isaksen (Reviewer's comment ID #: 115-2)]	Accepted. Text modify.
4-42	A	2:16		of course this relationship works both ways - with reduced snow cover, there will likely be warmer temperatures, both from the standpoint of albedo change as well as lack of melting that keeps temperatures nearer to 0°C. These points are noted later, but some comment about the interactive nature of the feedback may be appropriate here. [David Rind (Reviewer's comment ID #: 214-31)]	Accepted. Text modified.
4-43	A	2:16		Of course this relationship works both ways - with reduced snow cover, there will likely be warmer temperatures, both from the standpoint of albedo change as well as lack of melting that keeps temperatures nearer to 0°C. These points are noted later, but some comment about the interactive nature of the feedback may be appropriate here. [Govt. of United States of America (Reviewer's comment ID #: 2023-268)]	See 4-42.
4-44	A	2:18		correlation coefficient is not necessary here. Just "weak correlation was found" works. [Kenichi Matsuoka (Reviewer's comment ID #: 172-1)]	1.Accepted. Number deleted 2. Rejected. Correlation is strong.
4-45	A	2:19		is low "elevation" correct? Isn't it low "latitudes"? Both regions are mountain regions. [Kenichi Matsuoka (Reviewer's comment ID #: 172-2)]	ACCEPTED in part. Elevation is correct but the text has been clarified.
4-46	A	2:23	2:23	Are you using one standard deviationo two? If it is only one, then double the confidence figures in "5.8±1.9" to "5.8±3.8" [VINCENT GRAY (Reviewer's comment ID #: 88-516)]	See 4-40.
4-47	A	2:24	2:24	Are you using one standard deviationo two? If it is only one, then double the confidence figures for " 6.5±1.4" to 6.5±2.8" [VINCENT GRAY (Reviewer's comment ID #: 88-517)]	See 4-40.

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4-48	A	2:26	2:26	Are you using one standard deviationo two? If it is only one, then double the confidence figures for " 2.7±0.7" to "2.7±1.4" [VINCENT GRAY (Reviewer's comment ID #: 88-518)]	See 4-40.
4-49	A	2:28	2:28	Are you using one standard deviationo two? If it is only one, then double the confidence figures for " 7.4±2.9" to "7.4±5.8" [VINCENT GRAY (Reviewer's comment ID #: 88-519)]	See 4-40.
4-50	A	2:28	2:28	Exec. Summary - Should certainly add that the record-low summer ice-cover minima have occurred over the most recent years (2002-05) [Ola M. Johannessen (Reviewer's comment ID #: 119-2)]	Rejected. Longer term changes are more important than individual years.
4-51	A	2:32	2:34	Exec. Summary - In contrast to the other variables, sea-ice thickness observations are conspicuously not up to date (only to 1997). This bullet implies that we can't say anything about the present decade. At the very least, we should add a sentence about the dearth of direct ice thickness measurements for the most recent years. We could also say that indirect indicators, e.g., the several % negative trends in multi-year area / perennial ice extent, suggest a probable reduction in ice thickness. [Ola M. Johannessen (Reviewer's comment ID #: 119-3)]	Rejected. All available direct measurements of ice thickness have been considered in our assessment.
4-52	A	2:36	2:36	Are you using one standard deviationo two? If it is only one, then double the confidence figures for "0.51±0.32" to "0.51±0.64" [VINCENT GRAY (Reviewer's comment ID #: 88-520)]	See 4-40.
4-53	A	2:36	2:40	Rates are quoted here in units of mm/year. It would be more consistent with Chapter 3 (where temperature trends, in particular, are quoted in K/decade) if the SLE changes were to be quoted in mm/decade. [Adrian Simmons (Reviewer's comment ID #: 242-80)]	Rejected. mm yr^{-1} is used in Ch. 5, 10
4-54	A	2:37	2:37	Are you using one standard deviationo two? If it is only one, then double the confidence figures for "0.81±0.43" to "0.81±0.86" [VINCENT GRAY (Reviewer's comment ID #: 88-521)]	See 4-40.
4-55	A	2:39	2:39	Insert after "Canada" "There were, hovever, mass gains in Canada, the Alps, Himalayas, Mt Kenya and New Zealand (Box 6.3. Figure 1)" [VINCENT GRAY (Reviewer's comment ID #: 88-557)]	Rejected. Not true for the recent past.
.4-56	A	2:39	2:39	add at the end of the line: ....large areas and volumes,..... [Atle Nesje (Reviewer's comment ID #: 190-1)]	Rejected.
4-57	A	2:42	2:42	Delete from "Taken together" to "shrinking". It is absurd to take these two different regions together [VINCENT GRAY (Reviewer's comment ID #: 88-522)]	Text clarified.
4-58	A	2:42	2:50	Consideration needs to be given to adding the ES of this Chapter a point on the likely cause of the acceleration of Amundsen sea ice streams eg on page 27 lines 11-28 the issue	Noted. ES is focussing on key findings.

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				is related to basal melting and increased ocean temperatures. Given the significance of this issue in the context of questions of the stability of the WAIS (See Vaughan 2006 in press) it would be very relevant. [William Hare (Reviewer's comment ID #: 99-34)]	
4-59	A	2:42	2:43	Exec. Summary - The first sentence which states that "thickening in central regions of Greenland is more than offset by increased melting near the coast" is too strong and does not reflect the uncertainly in the estimates nor the high interannual variability of mass gains and losses. [Ola M. Johannessen (Reviewer's comment ID #: 119-4)]	Rejected. There is enough scientific evidence to support the text.
4-60	A	2:42	2:50	Please reconcile the apparent current positive contribution from Antarctica with the projected negative contribution to sea level rise from Antarctica in Chapter 10. [Govt. of United States of America (Reviewer's comment ID #: 2023-269)]	The current estimate of Ch. 4 is a mass loss of Antarctica contributing 0.2+/- 0.35 mm per year to sea level rise, which arises mostly from dynamical processes, such as accelerating glaciers and ice streams. Current coupled climate models cannot adequately treat these dynamical processes in detail. As a consequence there is a discrepancy to the future projection of a negative contribution to sea level rise, which is discussed in Ch. 10. A negative contribution to sea level rise is predicted from all climate models as a consequence of increased surface accumulation in connection with Antarctic warming. So far, Antarctic warming (over the continent in general) and increased precipitation have not been observed. This is commented on in Ch. 10. Also discussed is increased ice discharge and whether it could outweigh the increased accumulation. If the current dynamical imbalance scaled up with global average temperature, it would approximately balance the increased accumulation.
4-61	A	2:43	2:43	Replace ".is more than offset" by "more than offsets". Zwally et al 2005 have shown that	Rejected. There are more estimates. See

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				this is so. Your statement is wrong [VINCENT GRAY (Reviewer's comment ID #: 88-523)]	Fig. 4.6.2.
4-62	A	2:45	2:47	Replace from "between" in line 45 to "2005" in line 47 with "+11±3Gt from 1992 to 2002" This is from Zwally et al 2005. [VINCENT GRAY (Reviewer's comment ID #: 88-524)]	Rejected. There are more estimates. See Fig. 4.6.2.
4-63	A	2:46	3:10	lack of "-" or "~" between two years. E.g. 1961<space> 2003 should be 1961 - 2003. [Kenichi Matsuoka (Reviewer's comment ID #: 172-3)]	The WORD-version is fine. Conversion problem (pdf)
4-64	A	2:48	2:48	Delete "and" [VINCENT GRAY (Reviewer's comment ID #: 88-525)]	Rejected.
4-65	A	2:49	2:49	Insert after ."2003". "-31±12Gt for 1992-2002". This again, the most accurate, from Zwally et al 2005 [VINCENT GRAY (Reviewer's comment ID #: 88-526)]	Rejected. There are more estimates. See Fig. 4.6.2.
4-66	A	2:49	2:50	Delete from "Axcceleration on line 49 to "Greenland" on line 50. The statement is untrue. [VINCENT GRAY (Reviewer's comment ID #: 88-527)]	Sentence deleted.
4-67	A	2:49		add "in Antarctic peninsula" after Acceleration of mass loss may have occurred" [Kenichi Matsuoka (Reviewer's comment ID #: 172-4)]	Sentence deleted.
4-68	A	2:52	2:52	"The depth at which these increases in permafrost temperature are observed should be provided. Greater increases in temperature will occur at shallower depths and changes in temperature in the upper few metres of the ground will reflect more recent changes in climate than those observed at greater depths. The statement does not represent the changes in permafrost temperature that are observed and should indicate that there is a great deal of variability in the rate of change. At some sites very little change is observed over the last 20 years. The statement focusses on the maximum rate and gives no indication that there is a fair bit of variability. " [Govt. of Canada (Reviewer's comment ID #: 2004-144)]	Depth of the temperature measurements clarified. Discussion of further details is beyond the scope of the Executive Summary.
4-69	A	2:52	2:53	"The thickness of permafrost that these rates of thawing are associated with should be given as well as the time period as it is not clear if these changes have also occurred since the 1980s (see further comments below)" [Govt. of Canada (Reviewer's comment ID #: 2004-145)]	Accepted. Tet modified.
4-70	A	2:52	2:53	The indicated melt rates at the permafrost base probably relate to relatively thin/warm permafrost. The value for the Tibetan Plateau is enormous and difficult to believe (error in order of magnitude? Specific condition at an individual site?). [Wilfried Haeberli (Reviewer's comment ID #: 94-4)]	Accepted. Numbers corrected.
4-71	A	2:52	2:52	The depth at which these increases in permafrost temperature are observed should be	See 4-68

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				provided. Greater increases in temperature will occur at shallower depths and changes in temperature in the upper few metres of the ground will reflect more recent changes in climate than those observed at greater depths. The statement does not represent the changes in permafrost temperature that are observed and should indicate that there is a great deal of variability in the rate of change. At some sites very little change is observed over the last 20 years. The statement focusses on the maximum rate and gives no indication that there is a fair bit of variability. [Sharon Smith (Reviewer's comment ID #: 244-3)]	
4-72	A	2:52	2:53	The thickness of permafrost that these rates of thawing are associated with should be given as well as the time period as it is not clear if these changes have also occurred since the 1980s (see further comments below) [Sharon Smith (Reviewer's comment ID #: 244-4)]	See 4-69.
4-73	A	2:53	2:54	"Permafrost degradation may be leading to changes to the land surface in some regions but it is probably not correct to say that changes are widespread. The changes discussed refer to areas where surficial materials are ice-rich. Permafrost degradation could be occurring in areas of ice-poor permafrost (materials largely consisting of bedrock for example) but little impact is observed." [Govt. of Canada (Reviewer's comment ID #: 2004-146)]	Accepted. Text modified.
4-74	A	2:53	2:54	Permafrost degradation may be leading to changes to the land surface in some regions but it is probably not correct to say that changes are widespread. The changes discussed refer to areas where surficial materials are ice-rich. Permafrost degradation could be occurring in areas of ice-poor permafrost (materials largely consisting of bedrock for example) but little impact is observed. [Sharon Smith (Reviewer's comment ID #: 244-5)]	Accepted. Text modified.
4-75	A	2:57	3:1	"It is not clear over what time period the increase in seasonal thaw depth of 0.2 m in Russia has occurred." [Govt. of Canada (Reviewer's comment ID #: 2004-147)]	Accepted. Text modified.
4-76	A	2:57	3:1	It is not clear over what time period the increase in seasonal thaw depth of 0.2 m in Russia has occurred. [Sharon Smith (Reviewer's comment ID #: 244-6)]	See 4-75
4-77	A	3:1		Any idea why the onset date of freezing in Autumn in Eurasia advanced? [David Rind (Reviewer's comment ID #: 214-32)]	No.
4-78	A	3:5	3:10	At the end of this paragraph it may be useful to add a version of Table 4.8.1(Estimates of cryospheric contribution to sea level change) and to also mention, in conclusion the link between increase ice loss and warming mentioned at the beginning of the Excutive	Rejected. The executive Summary should not contain tables.

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				Summary [William Hare (Reviewer's comment ID #: 99-24)]	
4-79	A	3:6	3:6	Do these combined ranges take correlation between the components into account? That is, can one give the same probability interpretation (e.g., 90% confidence, if that is what is used) to these combined ranges as to the individual ranges. [Francis Zwiers (Reviewer's comment ID #: 305-4)]	Data base is insufficient to achieve this.
4-80	A	3:9	3:9	Are you using one standard deviationo two? If it is only one, then double the confidence figures for "1.2±0.6" to "1.2±1.2" [VINCENT GRAY (Reviewer's comment ID #: 88-528)]	See 4-40.
4-81	A	3:15	3:15	Insert before "changes" (with lower case), "Calculations suggest that" [VINCENT GRAY (Reviewer's comment ID #: 88-530)]	4:15 Rejected.
4-82	A	3:16	3:16	Delete "significantly" This usually has a statistical connotation, absent here [VINCENT GRAY (Reviewer's comment ID #: 88-531)]	4:16 Accepted. Text modified.
4-83	A	3:17	3:17	Delete "crucial" Don't exaggerate [VINCENT GRAY (Reviewer's comment ID #: 88-532)]	4:17 Rejected.
4-84	A	3:19	3:19	Delete "tiny" Don't exaggerate [VINCENT GRAY (Reviewer's comment ID #: 88-533)]	Rejected. Two orders of magnitde smaller is tiny.
4-85	A	4:10	4:11	It is often stated that the cryosphere is a sensitive indicator - but ice-albedo feedback amplifies both the response to forcing and the internal variability, with the result that the signal-to-noise ratio (and hence detectability) may be no better than elsewhere in the climate system.H16 [Francis Zwiers (Reviewer's comment ID #: 305-5)]	Accepted. Text modified for clarification
4-86	A	4:14		The statement is unclear. 75% counts only landed ice, or does it include sea, river, and lake ice that shows significant seasonal change? Because cryosphere is defined just several lines above Line 3 of this page) and seasonal variability is emphasized in the next paragraph, it is confusing for me. [Kenichi Matsuoka (Reviewer's comment ID #: 172-5)]	Text clarified.
4-87	A	4:19	4:20	Delete the following satement" only a tiny fraction lies in ice caps and glaciers outside". Add the following statement " the Antarctica and Greenland account for over 90% of ice mass on the earth (Table4.1.1). The glaciers and ice caps outside polar regions, only a tiny fraction of ice mass on the earth, however play important role in climate system." [Govt. of China (Reviewer's comment ID #: 2006-43)]	Rejected. The biggest impact of glaciers is on sea level. All other climate variables are not affected.
4-88	A	4:25		Replace "frozen ground" with "permafrost" as this is the relevant long-term condition. Chapter needs to review and be consistent about definition of frozen ground. [Govt. of United States of America (Reviewer's comment ID #: 2023-270)]	Accepted. Text modified.
4-89	A	4:26		Can you give a brief explanation about the role of sea ice and its seasonal variability?	Rejected. Beyond the scope of the

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				[Kenichi Matsuoka (Reviewer's comment ID #: 172-6)]	assessment.
4-90	A	4:30	4:43	Should the right hand column distinguish between zero (for sea-ice and ice-shelves), and unknown (for seasonally frozen ground and permafrost)? I have no idea whether thawing of the latter would contribute in a measurable way to ocean volume, but discussion in 4.7.4 (page 33, line 36) suggests impact on run-off. [Francis Zwiers (Reviewer's comment ID #: 305-6)]	Accepted. Table modified
4-91	A	4:42	4:43	give volume of ice beneath the current sea level. Numbers are given for other factors, which are relatively well known. [Kenichi Matsuoka (Reviewer's comment ID #: 172-7)]	Rejected. Numbers are given in cited publications.
4-92	A	4:46		First sentence should start with "we can see larger seasonal variability in snow-covered area" or something like that. Because seasonal variability is emphasize in the section just before, it leads readers confused. [Kenichi Matsuoka (Reviewer's comment ID #: 172-8)]	Rejected. There is nothing wrong with the present text.
4-93	A	5:1	5:3	In my opinion, it is room for a brief discussion of the relative importance of various feedbacks at low/high latitudes and in different seasons. [Roxana Bojariu (Reviewer's comment ID #: 24-2)]	Partially addressed. Further discussion beyond the scope of the assessment.
4-94	A	5:9	5:10	The sentence could be removed as is not relevant in the context of WG1. [Roxana Bojariu (Reviewer's comment ID #: 24-3)]	Accepted. Text modified.
4-95	A	5:24		Rewrite so that "Glaciers and ice caps adapt to a change of climate conditions much more rapidly than the ice sheets, because they have a relatively high ratio of…." [Kenichi Matsuoka (Reviewer's comment ID #: 172-9)]	Accepted. Text modified.
4-96	A	5:26	5:26	Add "as" after "such". [Chris Folland (Reviewer's comment ID #: 71-72)]	Accepted. Text modified.
4-97	A	5:26	5:26	Insert an "as" after "such". [Francis Zwiers (Reviewer's comment ID #: 305-7)]	Accepted. Text modified.
4-98	A	5:34		TAR??? [Kenichi Matsuoka (Reviewer's comment ID #: 172-10)]	Accepted. Abbreviation is expanded.
4-99	A	5:38	5:38	This line does not make sense. [Chris Folland (Reviewer's comment ID #: 71-73)]	Accepted. Text modified
4-100	A	5:38	5:38	"manifold" should perhaps be "many-fold" [James Renwick (Reviewer's comment ID #: 211-2)]	Accepted. Text modified.
4-101	A	5:40	5:48	The areas quoted here cannot be known this accurately surely, and the words approximately and about are inappropriate. The period of time when these areal estimates were made should be mentioned. [Chris Folland (Reviewer's comment ID #: 71-74)]	Accepted. Text modified.
4-102	A	5:43	5:44	Link between being the largest component and the most vulnerable is not clear.	Accepted. Half sentence deleted.

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				[Sharon Smith (Reviewer's comment ID #: 244-7)]	
4-103	A	5:44		The statement that frozen ground is the most vulnerable based simply on extent is debatable. I would think snow cover and sea ice cover are shown to be more susceptible to change. There is no criteria provided for vulnerability. Frozen ground is more resistant to change than snow. I would delete this conclusion here and in all other places. The ACIA report does not make this conclusion. [Govt. of United States of America (Reviewer's comment ID #: 2023-271)]	Accepted. Text modified.
4-104	A	5:45	5:46	"Permafrost records air temperature changes and other proxy information about environmental changes" This could be presented better as it is the permafrost temperatures (or in more general terms, the ground temperature) that reflect changes in temperature that occur at the ground surface. The ground surface temperature in turn reflects changes in air temperature and other climate variables such as snow cover and also environmental changes (eg. change in vegetation cover). [Sharon Smith (Reviewer's comment ID #: 244-8)]	Accepted. Text modified.
4-105	A	5:45	:48	The text says, "Frozen ground can translate climatic change to other environmental components and facilitate further climate change through the impacts on greenhouse gas exchange between the atmosphere and the land surface." The meaning of this in not clear. [Govt. of United States of America (Reviewer's comment ID #: 2023-272)]	Accepted. Text modified.
4-106	A	5:46	5:48	There are also links to moisture fluxes that should be mentioned. Changes in permafrost conditons for example may lead to changes in moisture availability for evaporation. [Sharon Smith (Reviewer's comment ID #: 244-9)]	Accepted. Text modified.
4-107	A	5:47		Remove "impacts on" [Govt. of United States of America (Reviewer's comment ID #: 2023-273)]	Rejected. But text is otherwise modified.
4-108	A	5:49		Could you add several sentences how these dynamic cryosphere components couples with each other, and other systems such as ocean and atmosphere? [Kenichi Matsuoka (Reviewer's comment ID #: 172-11)]	Rejected. Beyond the scope of the assessment.
4-109	A	6:4	6:9	Snow albedo has been linked to NAO variability as well (reference needed). The NAO is not itself an annular mode though it may be related to one. [Chris Folland (Reviewer's comment ID #: 71-75)]	TAKEN INTO ACCOUNT. For brevity the reader is referred to section 3.6.4.
4-110	A	6:5		It is good to see here a definition of what amounts to an indirect rather than a direct effect (more than two causal steps). Is this definition applicable to the whole report? There is discussion of direct and indirect radiative effects in Chapter 2; if a consistent definition is applied, perhaps this can be spelt out early on. [Adrian Simmons (Reviewer's comment ID #: 242-81)]	NOTED. Will mention to the editor of the Glossary and Chapter 2 CLAs.
4-111	A	6:8	6:9	In recent literature, the influences of snow cover on annular mode variability are identified elsewhere pure statistics, too. Numerical experiment with models show that	ACCEPTED. Text revised.

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				there is physics in the statistical relations (Gong et al., 2003: Modeled Northern Hemisphere Winter Climate Response to Realistic Siberian Snow Anomalies. J. Climate.). So, I would remove the sentence: "though these connections are statistically tenuous and controversial". [Roxana Bojariu (Reviewer's comment ID #: 24-4)]	
4-112	A	6:19	6:47	Section 4.2.2.1 (Sources of snow data) could probably be tightened up, if Ch. 4 needs to reduce its length. [Melinda Marquis (Reviewer's comment ID #: 162-27)]	ACCEPTED.
4-113	A	6:22	6:25	This sentence is too specific (USA / Canada). Delete or also discuss other continents. [Eric Martin (Reviewer's comment ID #: 166-1)]	ACCEPTED. Mountain observations are rare but Australia added.
4-114	A	6:52	6:52	What year or period does this estimate of snow cover refer to, as its changing. [Chris Folland (Reviewer's comment ID #: 71-76)]	ACCEPTED. Dates added.
4-115	A	6:56	6:56	"Non-summer" does not make sense as October already mentioned is a non summer month. [Chris Folland (Reviewer's comment ID #: 71-77)]	REJECTED. October is an exception; the text says "generallly" which allows exceptions.
4-116	A	7:1	7:2	This sentence does not flow from previous text. Trends were not mentioned in previous sentences [Chris Folland (Reviewer's comment ID #: 71-78)]	REJECTED. The previous paragraph was about variability. This paragraph is about trends.
4-117	A	7:17	7:24	It might be good to include in addition to/instead of Table 4.2.1 the figure on the whole NH Snow Cover Anomalies, e.g. http://climate.rutgers.edu/snowcover/chart_anom.php?ui_set=nhland or similar. [Govt. of Finland (Reviewer's comment ID #: 2009-56)]	NOTED. This table, and Figure 4.2.1, already refer to NH snow cover area, so it's not clear what the reviewer recommends.
4-118	A	7:17	7:23	Does the assessment of statistical significance take things like serial correlation into account? [Francis Zwiers (Reviewer's comment ID #: 305-8)]	ACCEPTED. Table now includes recalculated significance.
4-119	A	7:22	7:22	0.05% level. [Chris Folland (Reviewer's comment ID #: 71-79)]	REJECTED. It is the 0.05 (5%) level, not 0.05%.
4-120	A	7:26	7:26	"Warming" seems to be the wrong word. Do you mean "temperature variations"? [Chris Folland (Reviewer's comment ID #: 71-80)]	ACCEPTED. Text changed.
4-121	A	7:51	7:54	"is supported by regression" instead of "is demonstrated by regression analysis " [Roxana Bojariu (Reviewer's comment ID #: 24-5)]	ACCEPTED – 'demonstrated' replaced by 'shown'
4-122	A	7:52	7:52	Regression analysis of what against what? [Chris Folland (Reviewer's comment ID #: 71-81)]	ACCEPTED – text clarified
4-123	A	8:17	8:17	Its surprising there was a correlation of snow cover with temperature with a value of -0.5 but no trend in snow cover given the strong warming trend in Fig 3.2.10 of CH3. So this statement implicitly contradicts Fig. 3.2.10.	NOTED. Fig 3.2.10 shows trends since 1979; here the trends are since 1957, so they are not necessarily contradictory.

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				[Chris Folland (Reviewer's comment ID #: 71-82)]	
4-124	A	8:36	8:38	I found this sentence to be confusing. The suggestion seems to be that snow melt causes warming. [Francis Zwiers (Reviewer's comment ID #: 305-9)]	ACCEPTED. Sentence removed.
4-125	A	8:42	8:42	Significant declines when? [Chris Folland (Reviewer's comment ID #: 71-83)]	ACCEPTED. Year added.
4-126	A	8:50	8:51	Poor sentence; what did the study show and give a reference, or omit. [Chris Folland (Reviewer's comment ID #: 71-84)]	ACCEPTED. Added result and ref.
4-127	A	9:1	9:1	"the" should be "its". [Chris Folland (Reviewer's comment ID #: 71-85)]	Accepted
4-128	A	9:11	9:13	Did not fully understand this sentence. [Chris Folland (Reviewer's comment ID #: 71-86)]	Noted – text modified
4-129	A	9:27	9:27	Please replace "confidence" with "significance" - while the literature often refers to confidence levels in tests of hypothesis, the correct term is signficance levels. Standard tests of hypothesis allow one to reject a null hypothesis at a given significance level, but do not provide information about the confidence with which one can state that the null hypothesis is false (intuitively we would like to express results in that way - but that would require a Bayesian statistical analysis). [Francis Zwiers (Reviewer's comment ID #: 305-10)]	Accepted
4-130	A	9:28	9:28	The ES gives an uncertainty range for this figure (5.8 days/century), which presumably should be substantiated in the text. [Francis Zwiers (Reviewer's comment ID #: 305-11)]	Accepted
4-131	A	9:31	9:31	These diagrams seem rather out of date or has the decadal averaging been timed differently on the graph to e.g CH3 where the last year of an effective quasi decadal average is plotted. Is the middle year used here? [Chris Folland (Reviewer's comment ID #: 71-87)]	Noted – unfortunately this is the most recent published analysis of these data, and an update was not possible
4-132	A	9:35	9:35	Please delete …of the country…. [Govt. of Germany (Reviewer's comment ID #: 2011-8)]	Accepted
4-133	A	9:35		repeated phrase. [David Rind (Reviewer's comment ID #: 214-33)]	Accepted
4-134	A	9:35		Repeated phrase "...of the country". [Govt. of United States of America (Reviewer's comment ID #: 2023-274)]	Accepted
4-135	A	10:2	10:2	Cross reference the "ice-albedo" effect to the radiative forcing chapter for more detail. [Chris Folland (Reviewer's comment ID #: 71-88)]	Accepted (will cross reference)
4-136	A	10:29	10:30	The sentence is not relevant to WG1. [Roxana Bojariu (Reviewer's comment ID #: 24-6)]	Rejected – text does not present impacts but rather provides context

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4-137	A	10:37	10:39	I don't think the statement "The position of the ice edge .. is balanced by melt" generally holds true. It may be true for the extreme maximum extent in some areas but not the climatological mean extent. Observational and modelling studies of interannual variations in winter ice extent (and hence also the climatological winter maximum ice extent) clearly show that wind-induced ice drift (e.g Harangozo, 2006) is the major constraint on winter maximum extent rather than air temperature, not least in the Antarctic Peninsula area. Even in the mildest Antarctic regions melting in the marginal ice zone in the winter appears to be quite small and ice compaction contributes a lot to winter ice retreats. More extensive ice has a 'cooling' effect that then helps to keep the winter ice cover intact. Notice, for example, how far north the ice goes in the west Antarctic Peninsula despite this being the mildest Antarctic region. Also, in some parts of the Antarctic and also the Labrador Sea ocean currents are another major constraint on how far ice extends away from the land. [Steve Harangozo (Reviewer's comment ID #: 98-1)]	Accepted – this sentence has been removed
4-138	A	10:40	10:40	It would be good to refer to a text book on sea ice at the end of the section; e.g. I think there a good one by Peter Wadhams. [Chris Folland (Reviewer's comment ID #: 71-89)]	Rejected – text book reference not necessary for an assessment
4-139	A	10:42	11:33	Section 4.4.2 "Sea ice extent and concentration", especially sub-sections 4.4.2.1 and 4.4.2.2: This is one of the very most important parts of Ch 4, as there have been very well documented and dramatic changes in arctic sea ice coverage, upon which consensus has been reached. However, this section is too brief and limited to NASA work almost exclusively. The author appears to have ignored my previous review comments, many of which are re-formulated and stated below. [Ola M. Johannessen (Reviewer's comment ID #: 119-5)]	Noted -- we have not ignored any review comments; however this is an assessment, not a literature review. Where possible some changes to the text have been made.
4-140	A	10:44	11:9	Section 4.4.2.1 (Data sources and time periods covered [for sea ice]) could probably be tightened up, if Ch. 4 needs to reduce its length. [Melinda Marquis (Reviewer's comment ID #: 162-28)]	Noted – no major length reductions needed
4-141	A	10:45	:45	'Satellite passive microwave data' is better than 'passive microwave satellite data.' [Steve Harangozo (Reviewer's comment ID #: 98-2)]	Noted – text modified
4-142	A	10:48	:48	Reference should be made to a record of fast ice duration from the sub-Antarctic South Orkney islands starting in 1903 (Murphy et al. 1995). This indicates a negative trend of over seven days per decade.Murphy, E.J., A. Clarke, C. Symon and J. Priddle. (1995). Temporal variation in Antarctic sea-ice: analysis of a long term fast-ice record from the South Orkney Islands. Deep Sea. Res., 42, l045-1062. [Steve Harangozo (Reviewer's comment ID #: 98-3)]	Accepted – text modified
4-143	A	11:5	11:9	Section 4.4.2.1: The presentation of multi-year ice studies is incomplete, misleading, and biased to NASA work. The contention that passive microwave multi-year ice estimates	Noted – this is an assessment, not a literature review and so a complete

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				are "probably not a reliable climate indicator" is misleading. Indeed, multi-year ice area in winter has been successfully derived using algorithms other than NASA's. Comiso's (2002) roundabout way to get at multi-year ice area actually confimed the climatic trend in multi-year ice published by Johannessen et al. (1999) in Science. Therefore, one should replace lines 6-9 and INSERT: "Comparisons of passive and active microwave estimates of multi-year ice fraction indicate large differences (e.g., Kwok et al., 1996). However, recent studies have developed and applied passive-microwave algorithms for multi-year ice (MYI) area in winter. Johannessen et al. (1999) found a ~7% per decade reduction in the MYI area 1978-98, compared with ~2% per decade in the total ice area in winter. Belchansky et al. (2004 and 2005) subsequently used neural-network analysis to produce a 25-year (1979–2004) observational record of mid-winter MYI distributions, and found the resulting negative trend to be similar to Johannessen et al. (1999). These results are supported by Comiso's (2002) trend in summer minimum ice extent, which is by definition the MYI extent at the time of year." [Ola M. Johannessen (Reviewer's comment ID #: 119-6)]	historical account is not appropriate. However, the text has been revised to indicate that passive microwave retrievals for MY ice are improving, included reference to Johannessen et al. (1999)
4-144	A	11:11	11:22	Section 4.4.2.2: The presentation of hemispheric, regional and seasonal trends is also incomplete, misleading, and biased to NASA work and more specifically to the contributing author's (Comiso) own papers.. This is ironic, given the fact that it is NASA researchers who consistently resisted the notion of a decline in arctic sea ice, at the same time that non-NASA analyses (Chapman and Walsh (1993), Johannessen et al. (1995); Bjørgo et al. (1997); Johannessen et al. (1999) and others subsequently) presented compelling evidence, which was then later corroborated by NASA (See next few comments) [Ola M. Johannessen (Reviewer's comment ID #: 119-7)]	Rejected – space limitations in this assessment preclude a comprehensive review of all work done in this field.
4-145	A	11:13	11:13	There should be sentences added after ".. relatively constant." INSERT: "A trend analysis based on Scanning Multichannel Microwave Radiometer (SMMR) data found a slight negative trend in arctic sea-ice extent from 1978-87 (Gloersen and Campbell, 1991), a 3.2 × 104 km2 yr-1 decrease (2.4% per decade). Data from the subsequent Special Sensor Microwave/Imager (SSM/I) has provided the basis to follow up the SMMR trends. The Johannessen et al. (1995) analysis of SMMR and SSM/I records taken separately revealed a greater reduction in arctic sea-ice area and extent during the SSM/I period – decreases from 1987-94 were ~4% per decade compared to ~2.5% per decade from 1978-87. Merged SMMR–SSM/I time series have since been produced and analyzed, establishing the trends more robustly (Bjørgo et al., 1997; Cavalieri et al., 1997). Two independent analyses of merged SMMR–SSM/I data established the trend in arctic ice area and extent (1978-95) to be about –3.0 × 105 km2 per decade, corresponding to ~3% per decade (Bjørgo et al., 1997; Cavalieri et al., 1997). Since then, several studies have come to this consensus, e.g., Johannessen et al., (2004)."	Rejected – this is an assessment, not a review of the historical literature on the topic.

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				[Ola M. Johannessen (Reviewer's comment ID #: 119-9)]	
4-146	A	11:14	11:14	The sentence ending in "… and Antarctic changes." should refer to Cavalieri et al. (1997). [Ola M. Johannessen (Reviewer's comment ID #: 119-8)]	Rejected – this asymmetry is self-evident in all time series of hemispheric ice extent including those shown in Fig.4.4.1
4-147	A	11:22	11:22	There is a sentence that mentions region-to-region differences. There is clearly the need to expand on this point and to illustrate it with a figure mapping the spatial variability in trends. An updated version of such a figure from Johannessen et al. (2004) is uploaded/sent separately via e-mail. Therefore, INSERT: "Figure 4.4.2-add indicates the spatial patterns of the linear trends in winter (a) and summer (b) sea-ice concentration from 1979–2005, as updated by Johannessen et al. (2004). During this period, the decreases in winter (a) were most pronounced (as large as ~60%) in the Barents and Greenland Seas. In contrast, the summer decreases (b) have been greater than 60% in large areas of the Beaufort, Chukchi and East Siberian Seas. These summer patterns are in agreement with independent analyses of ice-cover minima (e.g., Comiso, 2002 and updated)." [Ola M. Johannessen (Reviewer's comment ID #: 119-10)]	Rejected – while we appreciate the reviewer providing the updated figure, space limitations preclude its incorporation.
4-148	A	11:25	11:25	Figs 4.4.1 shows a shorter satellite ice extent period than the TAR. I suggest that in addition to the Comiso data shown, HadISST sea ice extent data is shown, much as in the TAR, back to 1973. HadISST is kept up to date and the sea ice data is reasonably homogeneous. [Chris Folland (Reviewer's comment ID #: 71-90)]	Rejected – this figure shows continuous, homogenous data; the HadISST record is shown in Fig. 4.4.3
4-149	A	11:27	11:31	The first sentence "The most remarkable change ..... shown in Figure 4.4.2." is certainly important, though again it points only to the contributing author's own unpublished update. Serreze et al. (2003) and Stroeve et al. (2005) are the ones who reported and actually diagnosed the record low summer ice cover from 2002-04. Therefore, on line 28, after the ".... shown in Figure 4.4.2.", INSERT: "Summer 2002 set the 25-year record for minimum ice extent and area (Serreze et al., 2003), since surpassed in 2004 (Stroeve et al., 2005) and with the latest summer (2005) the lowest on record (Fig. 4.4.2)." [Ola M. Johannessen (Reviewer's comment ID #: 119-11)]	Rejected – in the context of this assessment the long-term trend is more relevant than the minima acheived in particular years.
4-150	A	11:33	11:33	Fig 4.4.2. Similar remarks.September HadISST satellite sea ice data could also be shown back to 1973. [Chris Folland (Reviewer's comment ID #: 71-91)]	Noted – as in comment 4-148, Fig. 4.4.3 will be modified to show HadISSt September time series.
4-151	A	11:35	12:2	Section 4.4.2.3, the part concerned with the Arctic. There is incomplete reporting of observed sea ice decreases in the early 20th century warming. Therefore, in this section, INSERT the sentence: "Johannessen et al. (2004) analyzed the Zakharov historical data set and showed pronounced and rapid decreases in sea ice during the 1920-30s warming	Noted – text will be modified slightly, however, historical Russian observations are already included in the figure and so this behaviour is already

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				period, though not as large as in those observed from satellite data during most recent, ongoing warm period." [Ola M. Johannessen (Reviewer's comment ID #: 119-12)]	illustrated.
4-152	A	11:47	11:49	"It is particularly notable that the Russian data indicate anomolously little ice during the 1940s and 1950s, whereas the Nordic sea data indicates anomolously large extent at this time." What is the point? That there can be great spatial variability in sea ice extent during a given period? If so, you might wish to say so explicitly. Does this have any implications for the extrapolations made in this assessment? [Melinda Marquis (Reviewer's comment ID #: 162-29)]	Noted – text modified
4-153	A	11:51	11:52	Greater variability would presumably be expected during cold periods, because I would except variability to be roughly proportional to the circumference of the ice-covered area, at least in cases where the ice covered area is not tightly confined by land on several sides. Thus the fact that this difference in variability is noticable might actually increase confidence in the data set. [Francis Zwiers (Reviewer's comment ID #: 305-12)]	Noted
4-154	A	11:54	11:54	"1800s" is ambiguous in this context. Do you mean nineteenth century? [Chris Folland (Reviewer's comment ID #: 71-92)]	Accepted – text modified
4-155	A	12:4	12:32	The actual words such as ice draft and freeboard should be defined or referred to the glossary the first time they are measured. [David Rind (Reviewer's comment ID #: 214-34)]	Accepted – text modified
4-156	A	12:4	:32	The actual words such as ice draft and freeboard should be defined or referred to the glossary the first time they are measured. [Govt. of United States of America (Reviewer's comment ID #: 2023-275)]	Accepted – text modified
4-157	A	12:22	12:22	Add "firm" before conclusions. It would be useful somewhere near this point in the text to cross refer to the climate change detection chapter for further insights about recent sea ice extent changes. [Chris Folland (Reviewer's comment ID #: 71-93)]	Accepted – text modified
4-158	A	12:26	12:51	Section 4.4.3.1 (Sea ice thickness data sources and time periods covered) could probably be tightened up, if Ch. 4 needs to reduce its length. [Melinda Marquis (Reviewer's comment ID #: 162-30)]	Noted – length is OK
4-159	A	12:27	12:29	Section 4.4.3.1. This sentence leaves the reader to wonder about the satellite remote sensing, to which reference should be made (Section 4.4.3.7). By the way, its placement there at the end may not be best -- why not simply include it in section 4.4.3.1? [Ola M. Johannessen (Reviewer's comment ID #: 119-13)]	Noted. Text will be modified slightly. Satellite methods were not included in 4.4.3.1 because they are still 'experimental'.
4-160	A	12:35	12:35	Unmatched bracket after "1958". [Francis Zwiers (Reviewer's comment ID #: 305-13)]	Accepted – text modified

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4-161	A	14:8	14:8	Explain what "landfast ice" is. [Chris Folland (Reviewer's comment ID #: 71-94)]	Rejected – term is define in text (section 4.4.1) and in glossary
4-162	A	14:16	14:16	Give a reference here. Is this multidecadal variability the Atlantic Multidecadal Oscillation phenomenon described in CH3, particularly in Knight et al (2005) referenced in CH3? [Chris Folland (Reviewer's comment ID #: 71-95)]	Noted – text modified. This is not related to AMO.
4-163	A	14:18	:19	Note comment 5 above about another fast-ice record for a sub-polar island. [Steve Harangozo (Reviewer's comment ID #: 98-4)]	Noted – text modified
4-164	A	14:19	14:19	Give the lats and longs of these stations. After "years", there should be a comma. [Chris Folland (Reviewer's comment ID #: 71-96)]	Noted – text modified
4-165	A	14:19	:20	Join the sentences starting "Although there' and 'At both sites'. [Steve Harangozo (Reviewer's comment ID #: 98-6)]	Noted – text modified
4-166	A	14:19		Incomplete sentence [David Rind (Reviewer's comment ID #: 214-35)]	Noted – text modified
4-167	A	14:19		Incomplete sentence. [Govt. of United States of America (Reviewer's comment ID #: 2023-276)]	Noted – text modified
4-168	A	14:44	14:44	Please insert the letter 'r' into "stong" [Govt. of Germany (Reviewer's comment ID #: 2011-9)]	Accepted – typo corrected
4-169	A	14:50	14:57	Should this paragraph be moved up? It seems inappropriate as part of a synthesis. [Francis Zwiers (Reviewer's comment ID #: 305-14)]	Accepted – text moved to section 4.4.3.3
4-170	A	15:12	15:28	Section 4.4.4.1 (Data sources and time periods covered [Pack Ice Motion]) could probably be tightened up, if Ch. 4 needs to reduce its length. [Melinda Marquis (Reviewer's comment ID #: 162-31)]	Noted – length OK
4-171	A	15:31	15:36	Give more detail about the basic character of these Arctic regimes. Cross refer to CH3 for more on the NAM. [Chris Folland (Reviewer's comment ID #: 71-97)]	Rejectd – detail about mode can be found in Glossary
4-172	A	15:34	:35	Use 1980s etc, not 1980's (check the whole text - its done correctly in some places including this page!) [Steve Harangozo (Reviewer's comment ID #: 98-10)]	Accepted – text modified
4-173	A	15:38	15:41	Is this variation related to ENSO? Or is it related to the Antarctic Circumpolar Wave? See also CH3 and cross ref if necessary. [Chris Folland (Reviewer's comment ID #: 71-98)]	Noted, text clarified.
4-174	A	15:52	15:53	Correlated in what direction? After "mean", add (presumably) "annual". [Chris Folland (Reviewer's comment ID #: 71-99)]	Accepted – text modified
4-175	A	16:1	16:1	Which record? Do you mean 24 years in length? To what end year? [Chris Folland (Reviewer's comment ID #: 71-100)]	Noted – text modified

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4-176	A	16:9		it might be useful to include - if available (IMO ?) - data (and trends) on ice berg (or pack ice) tracks, e.g. into the North Atlantic, Norway or Barents Sea, as a possible threat to navigation or oil installations. [Govt. of Germany (Reviewer's comment ID #: 2011-126)]	Rejected – this is a WG-II topic
4-177	A	16:9		I found this section to be substantially harder to read than the others. [Francis Zwiers (Reviewer's comment ID #: 305-15)]	Noted: Much of the text has been modified and simpplified
4-178	A	16:18	16:20	The sea level rise equivalents (SLE) are estimated to be about 40% higher than those given in the TAR. The reason(s) why should be explained. [Govt. of Japan (Reviewer's comment ID #: 2014-39)]	Taken into account: text modified to address the new data sets used.
4-179	A	16:24		Unit for SLE should be mm, not meters. [Kenichi Matsuoka (Reviewer's comment ID #: 172-12)]	Rejected: the comment is presumably based on a misunderstanding. The numbers given (in meters) are for the potential SLE of the entire mass of glaciers and ice caps.
4-180	A	16:42	16:44	Eliminate the entire sentence from "small and steep" to "flatter glaciers". The statement is at least problematic and confusing if not wrong. Horizontal effects from mountain shadow, wind drift and avalanches are especially important on small glaciers and the influence of vertical gradients still predominates on large glaciers. [Wilfried Haeberli (Reviewer's comment ID #: 94-5)]	Accepted. Text modified
4-181	A	16:45	16:46	Add ", temperate glaciers" (or ", firn and ice at melting temperature" after "(maritime - large mass turnover …)" and ", polythermal to cold glaciers" (or ", firn and ice below melting temperature") after "(continental - small mass turnover" …). [Wilfried Haeberli (Reviewer's comment ID #: 94-6)]	Rejected: too detailed for IPCC AR
4-182	A	16:46	16:47	What are "moisture related conditions"? [Chris Folland (Reviewer's comment ID #: 71-101)]	Taken into account: text modified to calrify
4-183	A	16:48	16:49	end of line 48, allow the mass is balanced. (better than say that mass balance toward zero) [Kenichi Matsuoka (Reviewer's comment ID #: 172-13)]	Taken into account. Text modified
4-184	A	16:49	16:49	What are the exceptions? Provide an example or omit. [Wilfried Haeberli (Reviewer's comment ID #: 94-7)]	Noted, however, to provide edequate information on the exceptions, e.g. surgiung glaciers, we would aquire a considerable extension of the text that is inappropriate for the assessment.
4-185	A	16:51	16:53	Provide a reference for this important statement: quantitative relations are given in Haeberli, W. and Hoelzle, M. (1995): Application of inventory data for estimating characteristics of and regional climate-change effects on mountain glaciers: a pilot study	Taken into account. appropriate references added

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				with the European Alps. Annals of Glaciology, 21, 206-212. Russian translation in: Data of Glaciological Studies, Moscow, 82, 116-124. [Wilfried Haeberli (Reviewer's comment ID #: 94-8)]	
4-186	A	16:51		lag of "several years" sounds too rapid. [Kenichi Matsuoka (Reviewer's comment ID #: 172-14)]	Rejected: in the Cordillera Blanca e.g., the lag is about 4 years. So, "several" is apropriate
4-187	A	17:2		The overall mass balance for glaciers and ice caps seems to be negative for the northern hemisphere (Arctic, Alaska, Greenland, Alps, Himalaya), neutral or slightly positive for the southern hemisphere (Antarctic). Could this imbalance between the hemispheres feed back to Earth rotation parameters (angular momentum; obliquity) ? Would it be worthwhile to discuss the amplitude of such an effect ? [Govt. of Germany (Reviewer's comment ID #: 2011-127)]	Noted: this issue is mentioned in section 4.6.2.1
4-188	A	17:4	17:5	Provide a reference which guides to the original data source and the corresponding internationally coordinated monitoring programme. Possibilities are (a) Haeberli, W. (2005): Mountain glaciers in global climate-related observing systems. In: Huber, U. M., Burgmann, H. K. H. and Reasoner, M. A. (eds): Global Change and Mountain Regions (A State of Knowledge Overview). Springer, Dordrecht, 169-175. (b) IUGG/UNEP/UNESCO (2005): Fluctuations of Glaciers 1995-2000 (ed. by Haeberli, W., Zemp, M., Frauenfelder, R., Hoelzle, M. and Kääb, A.) and erlier volumes. World Glacier Monitoring Service, Zurich, Switzerland. [Wilfried Haeberli (Reviewer's comment ID #: 94-9)]	Accepted: a reference is added
4-189	A	17:9	17:12	There needs to be a cross referral to CH3 here. The precipitation-driven glacial advances are due to the increase in the positive phase of the winter NAO in Scandinavia and to an increase in south west winds in the New Zealand Southern Alps, very likely related to more frequent ENSOs or to their multidecadal manifestation, the Interdecadal Pacific Oscillation. Please interact with CH3 on this. The latter point should be clarified through Dr Renwick of CH3. [Chris Folland (Reviewer's comment ID #: 71-102)]	Taken into account and discussed with Dr. Renwick. The issue is discussed in chapter 3.9.
4-190	A	17:11	17:11	Better write "growth" or "growth and advance" rather than only "advance. [Wilfried Haeberli (Reviewer's comment ID #: 94-10)]	Accepted: text modified
4-191	A	17:12	17:12	Add at end."Box 3. Figure 1 shows as many glaciers advancing as retreating" [VINCENT GRAY (Reviewer's comment ID #: 88-556)]	Rejected. Figure 1 only shows pentadal means. We don't understand the Box 3 comment.
4-192	A	17:37	17:37	Amend the figures for Greenland, using the figures found by Zwally et al 2005, who found that Greenland was increasing by +16±11Gt between 1992 and 2002 [VINCENT GRAY (Reviewer's comment ID #: 88-544)]	Rejected: there is a misunderstanding. This chapter section does not deal with the ice sheets that are the subject of Zwally et al. 2005, but only with

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					glaciers and ice caps neighbouring the ice sheets
4-193	A	17:51	17:51	"pentade" wont be generally understood. [Chris Folland (Reviewer's comment ID #: 71-103)]	Accepted: "pentade" is defined when first used.
4-194	A	18:11	18:11	I think this should be negative mean specific mass balances. [Chris Folland (Reviewer's comment ID #: 71-104)]	Accepted: text modified
4-195	A	18:26	18:28	this seems somewhat risky… [David Rind (Reviewer's comment ID #: 214-36)]	Noted: However Dyurgerov and Meier (2005) have expained howthis is the best estimate when there are no in situ data
4-196	A	18:26	:28	What are the implications of this assumption and inference? [Govt. of United States of America (Reviewer's comment ID #: 2023-277)]	see 4-195
4-197	A	18:27	18:27	I don't understand "has been adapted to the course of" [Chris Folland (Reviewer's comment ID #: 71-105)]	Taken into account. Text modified
4-198	A	18:41	18:41	"based on" probably means "as being due to" [Chris Folland (Reviewer's comment ID #: 71-106)]	Accepted: text modified
4-199	A	18:43	18:43	"effect" should be "result" [Chris Folland (Reviewer's comment ID #: 71-107)]	Accepted: text modified
4-200	A	19:1	19:3	How can Svalbard ice cap growth contribute anything to sea level rise? [Chris Folland (Reviewer's comment ID #: 71-108)]	Taken into account, text modified
4-201	A	19:2	19:3	unclear. This amount of the sea level contribution is from the Svalbird? If so, why is it positive? [Kenichi Matsuoka (Reviewer's comment ID #: 172-15)]	see 4-200
4-202	A	19:2		is this contribution negative? [David Rind (Reviewer's comment ID #: 214-37)]	see 4-200
4-203	A	19:2		Is this contribution negative? And use common units for mm per year. International System of Units. [Govt. of United States of America (Reviewer's comment ID #: 2023-278)]	see 4-200
4-204	A	19:8	19:9	A more moderate rate than what? Also explain poly-thermal glacier [Chris Folland (Reviewer's comment ID #: 71-109)]	Accepted: text modified
4-205	A	19:9	19:11	Here is a sentence which needs some clarification to make sense. I addressed this issue also on the last draft. In this section we can read that glaciers in Scandinavia is loosing mass and they all retreat. Then in the middle of the section we can read that the cold part of Storglaciären has thinned 8.3 m. The reader may get the impression that the glacier has thinned to such an extent which is certainly wrong. We have measured a change in the thermal distribution of Storglaciären over the last 15 years. Around 1990 the glacier	Accepted: changed following discussion with CA P. Jansson.

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				consisted of approximately 85% temperate ice, and 15% cold (below freezing point ice). This relation has changed in favour of the temperate ice. This has a strong climate relevance, but has little to do with shape and volume changes of glaciers. The best solution would be to make a separate section of this information and add one or two sentences explaining what is meant. A second possibility is to move this information into the permafrost section, but then it may need some more explaining text. [Per Holmund (Reviewer's comment ID #: 108-3)]	
4-206	A	19:16	19:20	Cross refer to CH3 and probably the detection and attribution chapter. [Chris Folland (Reviewer's comment ID #: 71-111)]	Accepted: cross reference made to Ch 3, Box 3.6.5
4-207	A	19:16	19:17	This comparison is not quite clear - would it be possible to use the same units in line 17 as in lines 16 and 18? [Francis Zwiers (Reviewer's comment ID #: 305-16)]	Accepted: text modified
4-208	A	19:17	19:17	What does "we" mean? [Chris Folland (Reviewer's comment ID #: 71-110)]	Noted, text modified
4-209	A	19:17	19:17	"… previous record of 1.6 m we loss in '1996' …" Should this "we" be deleted? [Melinda Marquis (Reviewer's comment ID #: 162-32)]	Noted, text modified
4-210	A	19:22	19:22	Replace the word " Himalayan" with the following satement" glaciers in the Tibetan Plateau and Himalayas" . Add" Yao et al., 2004" after "Solomina et al.,2004;" for reference ,see next comment. [Govt. of China (Reviewer's comment ID #: 2006-44)]	Noted, text modified
4-211	A	19:22	19:25	This paragraph on the Himalayan and Central Asian glaciated regions seems too brief considering its significance (Barnett, T. P., J. C. Adam, and D. P. Lettenmaier (2005). "Potential impacts of a warming climate on water availability in snow-dominated regions." Nature 438(7066): 303-306) and does not seem to characterise the general trend towards overall deglaciation that is apparent in the literature (See Lemkuhl and Owen (2005), Khromova et al (2003), Kulkarni et al (2003,2004) and others. I suggest an additional sentence like "In general glaciers in the Himalaya and other high mountain regions of Central Asia are retreating". [William Hare (Reviewer's comment ID #: 99-25)]	Rejected: the first paper mentioned exclusively deals with model projections and was suggested to WG2; the second paper deals with reconstructions of glacier extents and is an issue for Ch 6; the third paper represents only a small region without particular exceptions to the global picture, some of the findings are included in Dyurgerov and Meier (2005).
4-212	A	19:22	19:25	See: Khromova, T. E., M. B. Dyurgerov, et al. (2003). "Late-twentieth century changes in glacier extent in the Ak-shirak Range, Central Asia, determined from historical data and ASTER imagery." Geophysical Research Letters 30(16). [1] Global analysis of glacier regimes reveals widespread wastage since the late 1970s, with a marked acceleration in the late 1980s. We investigate changes in the heavily glacierized Ak-shirak Range, central Tien Shan plateau (43 degreesN, 75 degreesE) using	Rejected: see 4-211

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				air photo mapping surveys (1943 and 1977), an ASTER imagery (2001), and long term glaciological and meteorological observations. The wasting of the Ak-shirak glacier system features a decrease in average glacier size, and an increase in the area of outcrops. A small shrinkage during 1943 - 1977 was followed by a greater than 20% reduction during 1977 - 2001 in response to increases in summer and annual air temperature and decreases in annual precipitation. [William Hare (Reviewer's comment ID #: 99-26)]	
4-213	A	19:22	19:25	See: Lehmkuhl, F. and L. A. Owen (2005). "Late Quaternary glaciation of Tibet and the bordering mountains: a review." Boreas 34(2): 87-100. Abundant glacial geologic evidence present throughout Tibet and the bordering mountains shows that glaciers have oscillated many times throughout the late Quaternary. Yet the timing and extent of glacial advances is still highly debated. Recent studies, however, suggest that glaciation was most extensive prior to the last glacial cycle. Furthermore, these studies show that in many regions of Tibet and the Himalaya glaciation was generally more extensive during the earlier part of the last glacial cycle and was limited in extent during the global Last Glacial Maximum (marine oxygen isotope stage 2). Holocene glacial advances were also limited in extent, with glaciers advancing just a few kilometers from their present ice margins. In the monsoon-influenced regions, glaciation appears to be strongly controlled by changes in insolation that govern the geographical extent of the monsoon and consequently precipitation distribution. Monsoonal precipitation distribution strongly influences glacier mass balances, allowing glaciers in high altitude regions to advance during times of increased precipitation, which are associated with insolation maxima during glacial times. Furthermore, there are strong topographic controls on glaciation, particular in regions where there are rainshadow effects. It is likely that glaciers, influenced by the different climatic systems, behaved differently at different times. However, more detailed geomorphic and geochronological studies are needed to fully explore regional variations. Changes in glacial ice volume in Tibet and the bordering mountains were relatively small after the global LGM as compared to the Northern Hemisphere ice sheets. It is therefore unlikely that meltwater draining from Tibet and the bordering mountains during the Lateglacial and early Holocene would have been sufficient to affect oceanic circulation. However, changes in surface albedo may have influenced the dynamics of monsoonal systems and this may have important implications for global climate change. Drainage development, including lake level changes on the Tibetan plateau and adjacent regions has been strongly controlled by climatic oscillations on centennial, decadal and especially millennial timescales. Since the Little Ice Age, and particularly during this century, glaciers have been progressively retreating. This pattern is likely to continue throughout the 21st	Rejected: see 4-211

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				century, exacerbated by human-induced global warming. [William Hare (Reviewer's comment ID #: 99-27)]	
4-214	A	19:22	19:25	See: Kulkarni, A. V., B. P. Rathore, et al. (2004). "Monitoring of glacial mass balance in the Baspa basin using accumulation area ratio method." Current Science 86(1): 185-190. In the Himalaya, glacier and snow-melt form an important source of water into the North Indian rivers. However, this source of water is not permanent as glacial dimensions change with climate. One of the important parameters to model future changes in glacial extent is the mass balance. In this communication an attempt has been made to calculate the mass balance for 19 glaciers in Himachal Pradesh using accumulation area ratio (AAR) method. A regression relationship between AAR and specific mass balance was developed using field data from 1982 to 1988 for Shaune Garang glacier and 1976 to 1984 for Gor Garang glacier. Regression analysis suggests good correlation between AAR and specific mass balance with r(2) as 0.80. AAR for 2000 and 2001 was estimated for 19 glaciers in the Baspa basin by systematic weekly analysis of WiFS images of Indian Remote Sensing satellite from May to September. Mass balance was estimated during 2001 and 2002 for 19 glaciers in the basin, suggesting overall specific mass balance value of -90 and -78 cm, respectively. The investigations suggest a loss of 0.2347 km(3) of glacial ice in the last two years. The investigation has shown that four glaciers have no accumulation area, as these are located in lower-altitude zones. These glaciers are expected to face terminal retreat due to lack of formation of new ice. This is likely to pose serious problem of availability of water to many villages located in the Baspa basin. [William Hare (Reviewer's comment ID #: 99-28)]	Rejected: see 4-211
4-215	A	19:22	19:25	See: Kulkarni, A. V., B. P. Rathore, et al. (2005). "Alarming retreat of Parbati glacier, beas basin, Himachal pradesh." Current Science 88(11): 1844-1850. The Himalayas has one of the largest concentrations of glaciers outside the Polar regions. Various reports suggest that a significant number of mountain glaciers are shrinking due to climatic variations. In this communication, unusual retreat of the Parbati glacier in the Parbati river basin, Kullu district, Himachal Pradesh is reported. This is one of the largest glaciers in the valley. Satellite data of 1990, 1998, 2000 and 2001 are used in the investigation. The study has shown that the glacier had retreated 578 in between 1990 and 2001, almost 52 in per year. This rate of retreat was confirmed by field observations of glacier terminus in October 2003. Position of glacier snout was estimated by comparing its relative position with other features in field and in satellite images. In addition, position of the snout was also estimated using Global Positioning System. Compared to other glaciers in the Himalayas, this glacier is retreating at a high rate. This is possibly because the glacier is located in the lower altitude range. About 90% of the glacier is	Rejected: see 4-211

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				located in the altitude range lower than 5200 in; this is almost equal to the average altitude of the snow line at the end of the ablation season. The specific mass balance of the glacier is estimated using Accumulation Area Ratio method for a year 2001 as - 86 cm. The amount of retreat along with maximum length was predicted as 1461 in between 2001 and 2022, more than the present rate of retreat This suggests that the Parbati glacier will continue to retreat at an unusual rate and it will profoundly affect the availability of water in the basin. [William Hare (Reviewer's comment ID #: 99-29)]	
4-216	A	19:24	19:25	Explain in more detail "enhanced transport of moisture to high altitudes". [Chris Folland (Reviewer's comment ID #: 71-112)]	Accepted: text modified
4-217	A	19:30	19:33	So is there a clear, dominant, cause for the shrinkage of tropical glaciers? [Francis Zwiers (Reviewer's comment ID #: 305-17)]	Noted, text modified
4-218	A	19:39	19:39	Do you mean increased solar radiation or sunshine? [Chris Folland (Reviewer's comment ID #: 71-113)]	Noted, solar radiation is correct
4-219	A	19:39	19:39	The message is not clear here for non-specialists. Do we infer that glacier shrinkage on Kilimanjaro is just an unfortunate artifact of the geometry of the surfaces from which ice is being lost, and that if the shape of these surfaces had been different, the mass balance situation might also have been different? That is, nothing to do here with a warming atmosphere?+H30 [Francis Zwiers (Reviewer's comment ID #: 305-18)]	Noted, the answer to the reviewers question is Yes. Text modified to make this clearer.
4-220	A	19:42	19:43	This deceleration is at first sight surprising - explain in more detail. [Chris Folland (Reviewer's comment ID #: 71-114)]	Accepted, text modified
4-221	A	19:43		odd way of saying this...not sure what it means [David Rind (Reviewer's comment ID #: 214-38)]	Noted, text modified
4-222	A	19:43		Odd way of saying this...not sure what it means. [Govt. of United States of America (Reviewer's comment ID #: 2023-279)]	see 4-221
4-223	A	19:47	20:4	This whole paragraph repeats long-known basic text-book knowledge (especially from line 49 to line 57). Moreover, it reflects impacts rather than indications of climate change and, hence, is a topic of the corresponding chapters in WG II rather than WG I. Better eliminate or reduce to the last 5 lines (which comprise facts).. [Wilfried Haeberli (Reviewer's comment ID #: 94-11)]	Accepted: paragraph reduced to shorten text. Fig removed and suggested to WG2
4-224	A	20:0	24:	Somewhere in this section—probably at the beginning or end—you need a paragraph that pulls things together as well as lines 42-50 on page 2. SPM ought to quote lines 42-43 on page 2, but it's style is to reference specific sections in the chapters. [Govt. of United States of America (Reviewer's comment ID #: 2023-280)]	Accepted—text added as 4.6, before 4.6.1.

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4-225	A	20:8		What I am missing is some kind of overall summary of which parts of the ice-sheet various changes are occurring. Most people would guess that in Antarctica coastal areas are thinning because of downstream disturbances, while some parts of the intererior might be thickening because of changes in accumulation pattern. Similar comments for Greenland. The information is buried in here, but some sort of summarisation is needed. [Richard Hindmarsh (Reviewer's comment ID #: 106-11)]	Accepted—text added as 4.6, before 4.6.1.
4-226	A	20:8		There remains a question of how much comment should be put in about the modelling. Ice-sheet modelling isn't up to the level of atmosphere etc.; we have stalled on some tricky points and the ice community (as a whole) deserves some criticism, and the rest of the community deserves some explanation. Richard Alley discusses this a bit at EGU - can he find a tactful way of putting it in here? [Richard Hindmarsh (Reviewer's comment ID #: 106-12)]	Accepted—text added as 4.6, before 4.6.1.
4-227	A	20:13	20:13	Does Lythe and Vaughan contain data for Greenland as well as Antarctica? [Richard Hindmarsh (Reviewer's comment ID #: 106-1)]	Accepted—reference added to Bamber et al. (2001) for Greenland.
4-228	A	20:13		This paper should be referred as Lythe, Vaughan and BEDMAP group (2001) [Kenichi Matsuoka (Reviewer's comment ID #: 172-16)]	Accepted—but this then becomes Lythe et al. (2001).
4-229	A	20:13		a modest change of WHAT? [Kenichi Matsuoka (Reviewer's comment ID #: 172-17)]	Accepted—text modified.
4-230	A	20:20		little melt in "continental" Antarctica (except for the peninsula). [Kenichi Matsuoka (Reviewer's comment ID #: 172-18)]	Accepted—text modified.
4-231	A	20:24		Besides speed variability of glaciers, it is also important to emphasize possible impact of such variations to the entire ice sheet. Of course it is still under investigation, but I don't want to provide an image that Antarctica is stable, although we don't know it yet. Probably, it is more informative to provide Bamber's Science paper in 2000 to indicate a possible dynamical consequence of such outlet glaciers/ice streams and continental ice sheet. [Kenichi Matsuoka (Reviewer's comment ID #: 172-19)]	Accepted—text modified.
4-232	A	20:35	20:36	well, it is true but it is a proven technique that should be referred here? If not, just drop it. [Kenichi Matsuoka (Reviewer's comment ID #: 172-21)]	Rejected—satellite gravity now widely used, through the GRACE mission.
4-233	A	20:35		repeated altimetry assesses height (elevation) changes, not volume changes. [Kenichi Matsuoka (Reviewer's comment ID #: 172-20)]	Accepted—text modified.
4-234	A	20:39		Section: (i) Mass-budget. In the discussion of the accumulation estimates the issues involved with accurate measurement of this term could be further spelt out. See the paper by Frezzotti (2004) on the SMB estimates in a region of East Antarctica. [William Hare (Reviewer's comment ID #: 99-30)]	Partially accepted—text modified in section noted.
4-235	A	20:39		Reference: Frezzotti, M., M. Pourchet, et al. (2004). New estimations of precipitation and	Partially accepted—this is a

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				surface sublimation in East Antarctica from snow accumulation measurements. Climate Dynamics. 23: 803-813. Abstract: Surface mass balance (SMB) distribution and its temporal and spatial variability is an essential input parameter in mass balance studies. Different methods were used, compared and integrated (stake farms, ice cores, snow radar, surface morphology, remote sensing) at eight sites along a transect from Terra Nova Bay (TNB) to Dome C (DC) (East Antarctica), to provide detailed information on the SMB. Spatial variability measurements show that the measured maximum snow accumulation (SA) in a 15 km area is well correlated to firn temperature. Wind-driven sublimation processes, controlled by the surface slope in the wind direction, have a huge impact (up to 85% of snow precipitation) on SMB and are significant in terms of past, present and future SMB evaluations. The snow redistribution process is local and has a strong impact on the annual variability of accumulation. The spatial variability of SMB at the kilometre scale is one order of magnitude higher than its temporal variability (20-30%) at the centennial time scale. This high spatial variability is due to wind-driven sublimation. Compared with our SMB calculations, previous compilations generally over-estimate SMB, up to 65% in some areas. [William Hare (Reviewer's comment ID #: 99-31)]	continuation of comment 4.234, and is dealt with there.
4-236	A	20:40		can you be a little bit more specific? This description does not so much make sense for me. I would suggest "with interpretation between core sites from satellite-measured microwave brightness that reflects the recent mass balance and/or from ice-penetrating-radar-detected depths of isochrones for a longer time scale. [Kenichi Matsuoka (Reviewer's comment ID #: 172-22)]	Rejected—space limitations preclude this level of specificity in referenced review material.
4-237	A	20:45		add "from satellite just before Rignot. [Kenichi Matsuoka (Reviewer's comment ID #: 172-23)]	Accepted—text modified.
4-238	A	20:52		inferred from "residual, provided that mass input and output measured with other methods should be balanced". [Kenichi Matsuoka (Reviewer's comment ID #: 172-24)]	Partially accepted—text modified.
4-239	A	20:54		delete "large". [Kenichi Matsuoka (Reviewer's comment ID #: 172-25)]	Accepted—text modified.
4-240	A	21:1	21:1	This is a poor sentence e.g "probably uncertain" Give a reference. [Chris Folland (Reviewer's comment ID #: 71-115)]	Accepted—text modified and reference added.
4-241	A	21:8	21:8	"an" before "increase". [Chris Folland (Reviewer's comment ID #: 71-116)]	Accepted—text modified.
4-242	A	21:13		insert "primary caused by postglacial rebound" after bedrock elevation. [Kenichi Matsuoka (Reviewer's comment ID #: 172-26)]	Partially accepted—text added in 4.6.2.1 (iii)
4-243	A	21:19		I would like to see a statement about year-to-year variation of accumulation. Measurements of two specific timings do not give a trend of a period ending with these	Rejected. Variability in accumulation rate is discussed on 21:7-8, just above,

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				two measurements. I am afraid that a statement just below (satellite altimetry data is available for a long-term period...) might lead readers to a misunderstanding. [Kenichi Matsuoka (Reviewer's comment ID #: 172-27)]	The issue of trends versus fluctuations is on 23:27-28 and elsewhere.
4-244	A	21:23		Refer Figure 4.6.3. This figure clearly shows the limitation of ERS altimetry (lack of south pole and steep area coverage). [Kenichi Matsuoka (Reviewer's comment ID #: 172-28)]	Accepted—text modified.
4-245	A	21:33	21:33	"the" before "signal" [Chris Folland (Reviewer's comment ID #: 71-117)]	Accepted—text modified.
4-246	A	21:56		Altimetry is also an geodetic measurement, right? I would like to have a section heading of "gravity measurements from satellites". Recently, capability of the gravity measurement was proven, while others are not. [Kenichi Matsuoka (Reviewer's comment ID #: 172-29)]	Rejected. Splitting gravity from other geodetic results in this section would have left very short subsections, taking extra room without gaining much.
4-247	A	22:11	22:16	I propose to delete this paragraph. [Kenichi Matsuoka (Reviewer's comment ID #: 172-30)]	Rejected. The well-known Munk result was not strongly consistent with other results; the newer work showing that the Munk constraints can be broadened and include other results here is important in the assessment.
4-248	A	22:13	22:13	"rotation vector" Explain in more detail. [Chris Folland (Reviewer's comment ID #: 71-118)]	Accepted—text modified.
4-249	A	22:21	23:28	Section 4.6.2.3, Greenland, and maybe elsewhere in the chapter. A possible explanation of the recent (last decade) big increase of low level land ice ablation in south and south east Greenland is a big increase in SST near the coast in recent years. It would be worth exploring this with the help of CH3 (D. Parker). Thus a time series of SST in a few 5x5 boxes near Cape Farewell (e.g the HadSST2 dataset) may show a relatively sudden and large SST increase in the late 1990s and 2000s. If this is true, CH5 needs to be informed. [Chris Folland (Reviewer's comment ID #: 71-119)]	Noted.
4-250	A	22:23	22:23	Delete "slight" [VINCENT GRAY (Reviewer's comment ID #: 88-534)]	Accepted—text modified.
4-251	A	22:23	22:23	Delete "strong" [VINCENT GRAY (Reviewer's comment ID #: 88-535)]	Accepted—text modified.
4-252	A	22:23	22:23	Replace "2006" with "2005" [VINCENT GRAY (Reviewer's comment ID #: 88-537)]	Accepted—text modified.
4-253	A	22:25	22:25	Add at end "Zwally et al.(2005) found a thinnng of the Greenland ice sheet at the margins of -42±2 Gt , but a growing, inland, of +53±2Gt; a net increase of +11±3". . [VINCENT GRAY (Reviewer's comment ID #: 88-536)]	Rejected—Zwally et al. (2005) results are discussed on 22:41-46, including likelihood that they show thinning

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					when density changes are included fully.
4-254	A	22:31		"near the coast" mean ice caps surrounding by the ice sheet? If so, it should be included in the ice-sheet mass balance. Otherwise, it should clearly state that isolated ice caps close to ice sheets. [Kenichi Matsuoka (Reviewer's comment ID #: 172-31)]	Rejected—"isolated" as the first word in the line shows that the ice caps are not part of the ice sheet.
4-255	A	22:51	22:55	What does a sentence "because they PROBABLY do not take account" mean? If it is not confirmed, don't state it. Otherwise, remove probably. [Kenichi Matsuoka (Reviewer's comment ID #: 172-32)]	Accepted—text modified.
4-256	A	23:25	23:28	The mention of interannual variability is highly welcome, but not complete. On line 27, after the sentence ending ".. Kangaratnam, 2006", add the sentence: "Moreover, interannual variability in winter elevation changes derived from SRALT have been shown to be linked to the NAO index (r ~ –0.9, lagged one month) during a study period 1992–2003 (Johannessen et al., 2005). For example, the extreme NAO reversal between 1995 to 1996 resulted in a 20 cm increase, which represents over +200 km3 in one year." [Ola M. Johannessen (Reviewer's comment ID #: 119-14)]	Rejected—attribution to NAO or similar influences is beyond the scope of the chapter, and the correlation noted is only for wintertime, not for annual averages considered here.
4-257	A	23:39	23:39	Replace "(in press)" with "(2005)" [VINCENT GRAY (Reviewer's comment ID #: 88-538)]	Accepted—text modified.
4-258	A	23:55	23:55	What are "input-output" techniques? [Chris Folland (Reviewer's comment ID #: 71-120)]	Accepted—text modified.
4-259	A	23:55	23:55	"input-output techniques" is meaningless jargon - all Antarcic mass balance is comparing input and output. [Richard Hindmarsh (Reviewer's comment ID #: 106-2)]	Accepted—text modified.
4-260	A	23:56	23:56	When did the Wordie Ice Shelf disappear? [Chris Folland (Reviewer's comment ID #: 71-121)]	Accepted—text modified to show strong shrinkage between 1966 and 1989.
4-261	A	24:11	24:11	"homogeneous" is not the right word - "uniform" is more appropriate [Richard Hindmarsh (Reviewer's comment ID #: 106-3)]	Accepted—text modified.
4-262	A	24:20	24:20	"GRACE data indicate loss of 139 ± 73 Gt a–1 between April, 2002 and July, 2005". It is misleading to compare GRACE data directly with the satellite interferometry. It would be better to state "GRACE data combined with models of Antarctic deglaciation…." [Richard Hindmarsh (Reviewer's comment ID #: 106-4)]	Rejected—errors associated with techniques are discussed earlier, for this 22:4-7.
4-263	A	24:24	24:24	State why formal error bars are not warranted, given that all the above estimates are given with error bars. [Richard Hindmarsh (Reviewer's comment ID #: 106-5)]	Accepted—text modified.
4-264	A	24:38	24:38	Statements like "melting of land ice" are always a bit ambiguous - they suggest direct	Accepted—text modified.

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				melting of the ice on land, and thus apparently exclude the melting of icebergs and by inference discharge from Antarctica and large areas of Greenland. [Richard Hindmarsh (Reviewer's comment ID #: 106-6)]	
4-265	A	24:40	24:40	Consistent with what? [Chris Folland (Reviewer's comment ID #: 71-122)]	Accepted—text modified.
4-266	A	24:42		Note that, once ice is afloat, it already contributed to the sea level. However, ice shelf can stabilize the ice sheet. [Kenichi Matsuoka (Reviewer's comment ID #: 172-33)]	Accepted—text modified.
4-267	A	24:43	24:47	Probably need an upfront statement here that changes in ice shelves do not directly affect sea-level. [Richard Hindmarsh (Reviewer's comment ID #: 106-7)]	Accepted—text modified.
4-268	A	25:6	25:32	Section: 4.6.3.1 Changes in snowfall and surface melting. A mention of observed melt areas over Antarctica and its surrounding ice shelves is warranted here (See Liu et al 2006 abstract below). Trends are not significant although the melting observed over some of the ice shelves may be of concern in relation to future stability. A strong temperature dependence on the areal extent of melt is found by Liu et al. [William Hare (Reviewer's comment ID #: 99-32)]	Rejected—surface melting of ice shelves is discussed in Ch. 10.
4-269	A	25:6	25:32	Reference: Liu, H., L. Wang, et al. (2006). "Spatiotemporal variations of snowmelt in Antarctica derived from satellite scanning multichannel microwave radiometer and Special Sensor Microwave Imager data (1978–2004)." J. Geophys. Res. 111(F1): 1-20. We derived the extent, onset date, end date, and duration of snowmelt in Antarctica from 1978 to 2004 using satellite passive microwave scanning multichannel microwave radiometer (SMMR) and Special Sensor Microwave Imager (SSM/I) data. A wavelet-transform-based method was developed to determine and characterize melt occurrences. About 9–12% of the Antarctic surface experiences melt annually. This is more than twice the surface melt extent measured in Greenland. Seasonally, surface melt primarily takes place in December, January, and February and peaks in early January. Regression analysis over the 25 year period of study reveals a negative interannual trend in surface melt. Nevertheless, the trend inference is not statistically significant. Large year-to-year fluctuations characterize the interannual variability. Extremely high melt occurred in the 1982/1983 and 1991/1992 summers, while extremely weak melt occurred in the 1999/2000 summer. A strong correlation with air temperature suggests that the melt index can serve as a diagnostic indicator for regional temperature variations. Periodic melting has been observed over Ross Ice Shelf, Ronne-Filchner Ice Shelf, the West Antarctic ice streams, and outlet glaciers in the Transantarctic Mountains. The Antarctic Peninsula, West Ice Shelf, Shackleton Ice Shelf, Amery Ice Shelf, and the ice shelf along the Princess Ragnhild Coast experienced the most persistent and intensive melt and should be	This is a continuation of comment 4-268, and is discussed there. .

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				closely monitored for their stability in the future, given the recent disintegration of the Larsen Ice Shelf A and B. [William Hare (Reviewer's comment ID #: 99-33)]	
4-270	A	25:20	25:20	Cross reference the Reanalyses to CH3 and ensure the same terminology for them is used. [Chris Folland (Reviewer's comment ID #: 71-123)]	Noted. Discussion self-contained here so cross-reference not added.
4-271	A	25:22	:23	In terms of melting, it is to be noted that summer rain precipitation days Turner et al (1997) have increased on the west Antarctic Peninsula coast since the 1950s. Rainfall rather than snow would accelerate melting. Also, the frequency of precipitation has increased (Turner et al., 2005). The latter finding was used to indirectly infer an increase in cyclonic activity in the region that could have contributed to reduced sea ice extent and to the warming in this part of the peninsula. Turner, J., T. Lachlan-Cope, S. Colwell, G.J. Marshall. 2005. A Positive Trend in Western Antarctic Peninsula Precipitation Over the Last 50 Years Reflecting Regional and Antarctic-wide Atmospheric Circulation Changes. Ann. Glaciol., 41, 85-91. Turner, J, S R Colwell and S A Harangozo (1997). Variability of precipitation over the western Antarctic Peninsula from synoptic observations. J. Geophys. Res., 102, 13999-14007. [Steve Harangozo (Reviewer's comment ID #: 98-5)]	Noted.
4-272	A	25:23	:32	I strongly feel this section could be more informative and that it needs clarifying and rewriting. At least in the case of annual mean surface air temperature, there are no statistically significant cooling trends in the Antarctic and surrounding ocean. Also, the warming trend in the Antarctic Peninsula is only greatest in winter on the west coast and islands (see http://www.antarctica.ac.uk/met/gjma). The discussion of Antarctic temperature trends starting 'Studies of ..' on l. 23 deserves to start on a new paragraph. It should be made clear that it is 'surface' warming that is being talked about (warming trends decrease considerably and quickly above the surface in the free troposphere). It should also be made clear that the strongest summer and winter warming trends in the Antarctic continent and sub-Antarctica occur in the Antarctic Peninsula but on the east and west coasts respectively. In the context of the IPCC report, the word 'reanalysis' should be avoided unless talking about GCM reanalyses (e.g. line 20). Thus 'A recent reanalysis (or study) of surface air temperature data poleward of ..' is fine. Likewise, reserve the word 'changes' for climate change and use something like (spatial) variations. Change overall warming for land, ocean and the whole domain' as its obvious it must be the whole domain so I suggest 'overall warming for Antarctica and the Southern Ocean'. I assume this overall warming is statisitically significant.I don't think the detailed point being made on lines 31 and 32 is	Partially accepted—text modified.

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				of great interest to an IPCC audience. We could identify warming and cooling trends in different periods in many station records – it's the long-term trend that matters for the IPCC. On line 26 replace 'especially' with generally. Three further points could be mentioned: 1] the warming in the western peninsula is the strongest in the southern hemisphere, 2] the peninsula is one of three polar regions that have the strongest surface warming globally, the others being Alaska and northern Eurasia and 3] Turner et al (2005) reported an increase in precipitation days on the west Antarctic Peninsula coast since the 1950s. The latter finding was used to indirectly infer increased cyclonic activity in the region that would contribute to reduced sea ice extent and to some of the warming in the Antarctic peninsula. [Steve Harangozo (Reviewer's comment ID #: 98-7)]	
4-273	A	25:23	:24	Change sentence on line 23-24 to, "Studies of Antarctic surface temperatures similarly show strong interannual variability linked to the major modes of Southern Hemisphere atmospheric circulation (Schneider et al, 2004)." Note that Schneider et al (2004) was not a trends study as implied in the draft text. [Govt. of United States of America (Reviewer's comment ID #: 2023-281)]	Partially accepted—text modified.
4-274	A	25:24	25:24	Cross reference Antarctic Peninsula warming to CH3 [Chris Folland (Reviewer's comment ID #: 71-124)]	Rejected.
4-275	A	25:25	:25	Sentence should start, "A recent trend analysis of temperature data poleward of 50 deg S from 1958-2002 shows overall annual mean warming for land. Ocean and the whole domain over that interval (Chapman and Walsh, in press)." Don't use "reanalysis" because it can be confused with the meteorological reanalysis, e.g. ERA-40, NCEP. [Govt. of United States of America (Reviewer's comment ID #: 2023-282)]	Partially accepted—text modified.
4-276	A	25:26	25:26	This new evidence for overall warming seems to contradict CH3 and needs to be discussed with them. [Chris Folland (Reviewer's comment ID #: 71-125)]	Noted.
4-277	A	25:28		Delete "and"; Start a new sentence with "These results [REFERRING TO CHAPMAN AND WALSH] are consistent with other recent studies, showing that the strongest trends are wintertime warming over the Antarctic Peninsula (ref. suggested: Turner, 2005) and the summer and autumn cooling over other areas of the continent (refs Doran et al, 2002; Thompson and Solomon, 2002)." [Govt. of United States of America (Reviewer's comment ID #: 2023-283)]	Partially accepted—text modified.
4-278	A	25:28	:30	Sentence starting with "Furthermore" is not very clear as to location or meaning of "full interval." The Chapman and Walsh study shows these claims are true for land only. Change sentence to: "Furthermore, annual mean trends on the Antarctic continent are dependent on the intervals considered, with trend analyses similarly ending in 2002, but	Partially accepted—text modified.

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				starting between 1966-1982 showing cooling, and starting between 1958-1965 showing warming." [Govt. of United States of America (Reviewer's comment ID #: 2023-284)]	
4-279	A	25:30		Delete the sentence starting with "Thus,." Now the cooling is covered with the sentence above. Suggested conclusion: "Thus, while the full 1958-2002 interval shows warming over Antarctica, it is important to consider the seasonality of the changes and the strong interannual variability when making interpretations." [Govt. of United States of America (Reviewer's comment ID #: 2023-285)]	Partially accepted—text modified.
4-280	A	25:44	25:45	Because Tamisiea et al., 2006, is being removed, this sentence will have to be altered. Unfortunately, this will probably remove the uncertainty estimate. [Mark Tamisiea (Reviewer's comment ID #: 262-3)]	Does not apply to Chaper 4 but to Chapter 5. Ch. 5 Response: The reference is removed, however we believe that the quoted uncertainty is indeed plausible.
4-281	A	25:48		Be specific. Stone's paper presents results only from Marybird land. Not presents results from the entire Antarctica. [Kenichi Matsuoka (Reviewer's comment ID #: 172-34)]	Rejected. "e.g." means "for example", and thus implies that this is not a complete list. Full referencing is not consistent with length limitations, and a comprehensive review of the rich literature isnot available.
4-282	A	25:53	25:53	I think this means "the acceleration of glacial ice-flows". [Chris Folland (Reviewer's comment ID #: 71-126)]	Partially accepted—text modified.
4-283	A	26:14	26:14	Add "Greenland" after Jacobshavn to remind the reader of where it is. Mixing Antarctica and Greenland in this way is a little confusing - should they have separate paragraphs? [Chris Folland (Reviewer's comment ID #: 71-127)]	Partially accepted—text modified.
4-284	A	26:20	26:24	Some statement about what Payne++ and Dupont & Alley predict would be useful [Richard Hindmarsh (Reviewer's comment ID #: 106-9)]	Rejected—Payne et al. and Dupont and Alley did not make predictions.
4-285	A	26:27	26:29	This sentence may be true, but the cause is more likely one of resolution; Hyubrechts model doesn't resolve the thinning events (in the Peninsula) alluded to above. I don't think that this sentence helps. [Richard Hindmarsh (Reviewer's comment ID #: 106-8)]	Accepted—text modified.
4-286	A	26:31	26:31	It would help to give the lat. and long. of the Helheim glacier. [Chris Folland (Reviewer's comment ID #: 71-128)]	Rejected—many sites are noted with references or general location information but not latitude-and-longitude coordinates; length restrictions preclude adding for all.
4-287	A	26:41	26:42	This increased sensitivity occurs under what forcing scenario?	Partially accepted—text modified.

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				[Francis Zwiers (Reviewer's comment ID #: 305-19)]	
4-288	A	27:1	:9	What's causing the warming on the eastern side of the northern Antarctic Peninsula? Reference needs to be made to Marshall et al. (2006) who used a high resolution regional climate model to link the summer warming on eastern side of the peninsula to the known strengthening of SAM at this time of the year. They went on to show that the more frequent westerly winds associated with a stronger SAM produce more frequent fohn winds as the westerlies flow over the mountains of the Antarctic Peninsula. Marshall GJ, Orr A, van Lipzig NPM, King JC. 2006. The impact of a changing Southern Hemisphere Annular Mode on Antarctic Peninsula summer temperatures. Journal of Climate. in press. A cross-reference to chapter 3 section 3.6.5 would help. [Steve Harangozo (Reviewer's comment ID #: 98-8)]	Rejected—not directly relevant or within the purview of chapter 4.
4-289	A	27:5	27:5	Cross refer to CH3, which seems to describe a larger Peninsula warming than this.. [Chris Folland (Reviewer's comment ID #: 71-129)]	Noted.
4-290	A	27:5	:5	Insert a reference to van den Broeke (2005) after the word 'melting' because he has confirmed how high surface air temperatures contributed to the melting leading to the breakup in 2002.van den Broeke, Michiel. 2005. Strong surface melting preceded collapse of Antarctic Peninsula ice shelf. Geophys. Res. Lett., Vol. 32, No. 12, L12815. [Steve Harangozo (Reviewer's comment ID #: 98-9)]	Rejected. Length limitations prevent comprehensive referencing.
4-291	A	27:14	27:17	Cross refer to CH3 for this warming It likely relates to Fig 3.2.7 and maybe Fig 3.6.8. Discuss with CH3 [Chris Folland (Reviewer's comment ID #: 71-130)]	Partially accepted—reference added to Thomas et al., 2003.
4-292	A	27:24	27:26	This should be made known to CH3 and CH5 which both need to consider this result. [Chris Folland (Reviewer's comment ID #: 71-131)]	Noted.
4-293	A	27:32		It is probably overstated that inland ice changes slowly. Well, we don't know about it. All we can say that we don't know how recent drastic changes in the coast region affect to the upstream region where it currently flows slowly. [Kenichi Matsuoka (Reviewer's comment ID #: 172-35)]	Accepted—text modified.
4-294	A	27:37	27:37	While the reference to Mitrovica et al., 2001, is appropriate, the attribution is wrong. The paper argues that the lower-than-expected values are due to ongoing melting from Greenland. [Mark Tamisiea (Reviewer's comment ID #: 262-2)]	Does not apply to Chaper 4 but to Chapter 5. Ch. 5 Response: Accepted, attribution removed.
4-295	A	27:38	27:39	surface melting is negligible for ice sheets. [Kenichi Matsuoka (Reviewer's comment ID #: 172-36)]	Rejected—statement is wrong, with Greenland losing half or more of snowfall by runoff of surface meltwater.
4-296	A	27:44		add a space between analogous and to.	Accepted—text modified.

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				[Kenichi Matsuoka (Reviewer's comment ID #: 172-37)]	
4-297	A	27:49		add "if the ice-flow model parameters are tuned so that ice models can give geometry consistent with geological evidence" after accuracy. [Kenichi Matsuoka (Reviewer's comment ID #: 172-38)]	Partially accepted—text modified.
4-298	A	27:52		typically above 2000 m elevation "of the ice sheet upper surface" [Kenichi Matsuoka (Reviewer's comment ID #: 172-39)]	Rejected—surface is specified immediately below
4-299	A	27:53		this is true in Greenland. Melting is much less significant in Antarctica. [Kenichi Matsuoka (Reviewer's comment ID #: 172-40)]	Rejected—limited surface melting also occurs in low-elevation regions of Antarctica..
4-300	A	27:56		and the "pressure" and distribution of water. [Kenichi Matsuoka (Reviewer's comment ID #: 172-41)]	Rejected—point is important, but probably unduly complicates what is supposed to be a simple presentation. .
4-301	A	28:1	28:1	Probably better to say "in only very few". [Richard Hindmarsh (Reviewer's comment ID #: 106-10)]	Partially accepted—text modified.
4-302	A	28:4		ice "walls" gives a correct information? I prefer to say that "ice sheets include areas with much higher ice-flow speed within it. Such high-speed ice, called ice streams, are controlled by glaciological and underneath geological characteristics." [Kenichi Matsuoka (Reviewer's comment ID #: 172-42)]	Rejected—text is correct.
4-303	A	28:8		unclear for me. [Kenichi Matsuoka (Reviewer's comment ID #: 172-43)]	Accepted—text modified.
4-304	A	28:31	28:32	I like to say that ice sheets and global temperature are coupled. Ice sheets do not passively respond to the climate change. Rather there are feedbacks. [Kenichi Matsuoka (Reviewer's comment ID #: 172-44)]	Noted.
4-305	A	28:42	28:42	Change title to "Changes in Permafrost and seasonally frozen ground" [Sharon Smith (Reviewer's comment ID #: 244-10)]	Rejected. Title is not to be changed. To be consistent with the chapter title. "Frozen Ground" is defined in the glossary.
4-306	A	28:42		As with the FOD, I am restricting my comments to Section 4.7, "Changes in Frozen Ground." The material has been improved very substantially, and now provides a well-rounded review of the state of the science of permafrost and seasonally frozen ground. The literature reviewed in the SOD is much more comprehensive than in earlier versions. This section could still stand a tight editorial review; I'm sure that this is in the works. If there is any space available, it would be good to add more material on periglacial geomorphology (e.g., ice-wedge polygons and other patterned ground, frost mounds (especially palsas), mass-wasting phenomena, and so forth). Other than a very brief treatment of thermokarst terrain and coastal erosion, this section lacks coverage of	Noted. Will add coverage of materials related with periglacial geomorphology.

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				geomorphic forms and processes, which are important aspects of the science of frozen ground, especially as it relates to climatic change. [Frederick Nelson (Reviewer's comment ID #: 188-6)]	
4-307	A	28:47	28:47	"areal" extent. [Chris Folland (Reviewer's comment ID #: 71-132)]	Accepted. Text modified.
4-308	A	28:47	28:47	change "area" to "areal" [Frederick Nelson (Reviewer's comment ID #: 188-1)]	Accepted. Text modified.
4-309	A	28:47	28:48	Link between being the largest component and the most vulnerable is not clear. [Sharon Smith (Reviewer's comment ID #: 244-11)]	Accepted. Text modified.
4-310	A	28:48	28:48	In what sense is frozen ground "vulnerable"? There is more of it than other cryosphere components, and thus there is potential to lose more of it, but vulnerability implies impacts of some kind - so the question is whether the loss of a large fraction of seasonally frozen ground would have a large climate (or other) impact. [Francis Zwiers (Reviewer's comment ID #: 305-20)]	Accepted. Text modified.
4-311	A	28:48		"hence the most vulnerable part". Being larger than anything else does not make something necessarily more vulnerable. Is the ice covering Antarctica more vulnerable to climate change than the smaller amount covering Greenland? The remark about vulnerability should be deleted or explained. [Adrian Simmons (Reviewer's comment ID #: 242-82)]	Accepted. Text modified.
4-312	A	28:48		Delete phrase about vulnerable because vulnerable not a defined term [Govt. of United States of America (Reviewer's comment ID #: 2023-286)]	Accpted. Text modified.
4-313	A	28:49	28:50	It would be better to say the atmospheric climate is an important factor. This would differentiates the larger scale effects from the other factors listed which will influence the microclimate. [Sharon Smith (Reviewer's comment ID #: 244-12)]	Noted.
4-314	A	28:53	28:53	Sentence should be "Thawing of ice-rich permafrost can …" It is important to indicate that it is ice-rich permafrost that is thaw sensitive (ie will exhibit subsidence upon thawing). It is also important to mention here that this thawing and settlement will occur in repsonse to surface disturbance and changes in climate. [Sharon Smith (Reviewer's comment ID #: 244-13)]	Accepted. Text modifed.
4-315	A	28:53	28:55	"dramatic" is a poor word choice. This sentence should be rewritten as there will not necessarily be changes in all things mentioned in all cases. For example, for some infrastructure there may be little change in performance as this will depend on its design etc. It may be better to say "...thermokarst that may lead to important changes in ecosystems,....." [Sharon Smith (Reviewer's comment ID #: 244-14)]	Accepted. Text modified.

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4-316	A	28:57	29:1	Again "dramatic" is a poor word choice. A better sentence would be: "Changes in permafrost conditions and seasonal freezing/thawing proccess can result in alterations to spatial patterns, sesonal to inter-annual variability and long-term trends in terrestrial carbon budgets....." [Sharon Smith (Reviewer's comment ID #: 244-15)]	Accepted. Text modified.
4-317	A	28:57	29:4	This is a pretty tough sentence to read - perhaps it can be re-written. It took me a while to figure out which spatial patterns, etc, were being discussed. [Francis Zwiers (Reviewer's comment ID #: 305-21)]	Accepted. Text modified.
4-318	A	29:1	29:1	spatial patterns of what? Seasonal to interannual variability of what? [Chris Folland (Reviewer's comment ID #: 71-133)]	Accepted. Text modified.
4-319	A	29:8		Section 4.7.2.1. A sentence should be added to this section that indicates that these various regional networks contribute to the Global Terrestrial Network for Permafrost (GTN-P). The earlier draft referred to this monitoring program. It is important to indicate that there are international efforts to maintain a long-term monitoring program to detect the climate signal in the cryosphere. Reference for GTN-P is Burgess, M.M., Smith, S.L., Brown, J., Romanovsky, V. and Hinkel, K., 2000. Global Terrestrial Network for Permafrost (GTNet-P): permafrost monitoring contributing to global climate observations; Geological Survey of Canada, Current Research 2000-E14; 8 p. [Sharon Smith (Reviewer's comment ID #: 244-19)]	Text modified. This assessment report will not identify any national and international programs and projects in the text.
4-320	A	29:9	29:18	How are the authors defining "deep permafrost temperatures"? and "shallow boreholes"? In referring to Russian sites, deep boreholes are deifined as deeper than 100 m. For description of Alasakan sites, shallow boreholes are generally <80m. In reference to northern Canada, the text indicates that monitoring of deep permafrost temperatures started in the early 1980s. Generally Canadian boreholes are less than 60 m deep with many less than 20 m deep so these would not be classified as deep. The Global Terrestrial Network for Permafrost (GTN-P) has 4 categories of boreholes: surface <10m, shallow 10-25m, Intermediate 25-125m and Deep geothermal >125m. The Canadian boreholes would be Shallow to Intermediate. [Sharon Smith (Reviewer's comment ID #: 244-17)]	Text modified.
4-321	A	29:10	29:11	Odd sentence. [Chris Folland (Reviewer's comment ID #: 71-134)]	Text modified.
4-322	A	29:10	29:10	Please insert the word 'Russian' between the words ...standard hydrometeorological…. [Govt. of Germany (Reviewer's comment ID #: 2011-10)]	Accepted. Text modified.
4-323	A	29:10	29:11	Reference to "standard hydrological stations" and "standard Russian Hydrological Stations" in the same sentence is a bit confusing. [Adrian Simmons (Reviewer's comment ID #: 242-83)]	Accepted. Text modified
4-324	A	29:10	29:10	remove "the standard hydrometeorological stations" ---- repetitive	Accepted. Text modifed.

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				[Sharon Smith (Reviewer's comment ID #: 244-16)]	
4-325	A	29:11	29:11	Please delete the words ... ' from the standard Russian Hydrometeorological Stations'… [Govt. of Germany (Reviewer's comment ID #: 2011-11)]	Accepted. Text modified.
4-326	A	29:16	29:26	Change the sentence to "….temperatures started mainly in the early 1980s in northern Canada." There are some sites where monitoring started earlier. [Sharon Smith (Reviewer's comment ID #: 244-18)]	Accepted. Text modified.
4-327	A	29:17	29:18	Reference papers that give scientific findings. Avoid references to PACE or other programs. I will not be identifying all of these throughout the chapter but would request the authors to please follow the approach discussed at the LA meetings to ensure that this report assesses the literature as required. Other organizations such as GCOS provide reports on research needs and organizations and programs but that is not appropriate here. [Susan Solomon (co-chair WG1) (Reviewer's comment ID #: 246-3)]	Accepted. Text modified.
4-328	A	29:17		Section 4.7.2.1 and Section 4.7.2.2: References of "Smith" are cited wrong at 4 places (Line: 17, 32, 35, 40) in this two sections. The two different authors "Smith", which have publications in the same year, should be distinguished by "a" and "b" after the year. For this two sections all should be cited: (Smith et al., 2005b) [Ketil Isaksen (Reviewer's comment ID #: 115-3)]	Accepted. Text modified.
4-329	A	29:18		Add new sentence "Romanovsky et al (2002) summarized results of many of these recent measurements. (add the following reference: Romanovsky, V. E., Burgess, M., Smith, S, Yoshikawa, K., and Brown, J., 2002. Permafrost temperature records: Indicator of climate change. Eos 83 (no. 50), pp.589, 593-594." [Govt. of United States of America (Reviewer's comment ID #: 2023-287)]	Accepted. Text modified and reference added.
4-330	A	29:20		Section 4.7.2.2. An initial summary of trends in permafrost temperatures was provided in Romanovsky et al. 2002. Perhaps this should be cited at the beginning of this section. Reference: Romanovsky, V., Burgess, M., Smith, S., Yoshikawa, K., and Brown, J. 2002. Permafrost temperature records: indicators of climate change; EOS, Transactions of the American Geophysical Union, v. 83 no. 50, p. 589. [Sharon Smith (Reviewer's comment ID #: 244-20)]	Accepted. Reference added.
4-331	A	29:22	29:24	Is the increase in temperature given the maximum? Should the sentence say that temperatures increased up to 2 to 4°C? - later in the sentence, the authors indicate that there was little warming or even cooling at some sites but the first part of the sentence would indicate that there was warming everywhere. [Sharon Smith (Reviewer's comment ID #: 244-21)]	Rejected. Here we stated in the sentence "in general" increased 2 to 4^{o}C.
4-332	A	29:22		Reword. Temperatures at the top of the permafrost [Govt. of United States of America (Reviewer's comment ID #: 2023-288)]	Accepted. Text modified.
4-333	A	29:24	29:27	If the rates given refer to the permafrost surface temperature throughout the paragraph, the	Accepted. Text modified.

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				authors should indicate this.Top of permafrost or permafrost table would be better terms to use than permafrost surface temperature. [Sharon Smith (Reviewer's comment ID #: 244-22)]	
4-334	A	29:24	29:24	Something missing between "increased" and "additional" - should this read "increase an additional"? [Francis Zwiers (Reviewer's comment ID #: 305-22)]	Accepted. Text modified.
4-335	A	29:29	29:29	Explain why increased snow cover has this effect on permafrost temperature [Chris Folland (Reviewer's comment ID #: 71-135)]	Accepted. Text modified.
4-336	A	29:32	29:33	The meaning of the phrase "The magnitude of the temperature increase reduced significantly…" is unclear [Frederick Nelson (Reviewer's comment ID #: 188-2)]	Accepted. Text modified.
4-337	A	29:42	29:42	A sentence could be added to the end of the paragraph that briefly describes the trend in permafrost temperatures in the Canadian High Arctic (note the Alert site is the most northerly site in the northern hemisphere) to give a more complete picture of the spatial variation in the Canadian permafrost zone. The sentence could be: Warming of permafrost at depths of 15 to 30 m since the mid 1990s has also been observed in the Canadian High Arctic (Smith et al. 2003). (note Smith et al. 2003 is already included in the reference list.) [Sharon Smith (Reviewer's comment ID #: 244-23)]	Accepted. Text modified.
4-338	A	29:44	29:47	Some of these results from Russia (I.e. from Pavlov 1996) would have been cited in TAR. Are there no recent results that would update information included in TAR for these sites? [Sharon Smith (Reviewer's comment ID #: 244-24)]	Noted.
4-339	A	29:48	29:49	The sentence states that termperatures from 3 central Siberian stations did not increase between 1991 and 2000 - Did they decrease or was there no apparent trend? Clarification is required. [Sharon Smith (Reviewer's comment ID #: 244-25)]	Accepted. Text modified.
4-340	A	29:52	29:55	Some small adjustments in the sentence and add correct reference: "Results from six years continuous ground temperature monitoring in a 102 m deep permafrost borehole on Janssonhaugen, Svalbard, indicate that the permafrost has warmed significantly, the mean annual ground temperature at a depth of 20 m currently increasing at a rate of about 0.5°C/decade (Isaksen et al., 2006).". --> Complete reference (please add to the reference list): "Isaksen, K., J.L. Sollid, P. Holmlund, and C. Harris, 2006: Recent warming of mountain permafrost in Svalbard and Scandinavia. J. Geophys. Res., Submitted." (Current Stage: With Editor for Decision) [Ketil Isaksen (Reviewer's comment ID #: 115-4)]	Accepted. Text modified.
4-341	A	29:52	30:22	Page 29 (lines 49-50) states, "... monitoring in the 100 m deep permafrost borehole on Janssonhaugen, Svalbard ..." and (lines 52-54) state, "...monitoring in Juvvasshøe, Southern Norway, indicate ... ground temperatures have increased by ~0.3°C at 15 m	Noted. Text modified.

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				depth." However, on page 30, Table 4.7.1, cites depth values for Janssonhaugen as ~5 m and for Juvvasshøe as ~5. Are these depth values in the table correct? Should they be revised to match those cited on page 4-29, e.g., should the row for Janssonhaugen, Svalbard cite 100 m depth and the row for Juvvasshøe, Southern Norway cite 15 m depth? [Melinda Marquis (Reviewer's comment ID #: 162-24)]	
4-342	A	29:53	29:54	It should be noted in the text that this increase over the past 60 to 80 years was determined through reconstruction of the ground surface temperature history from the deeper temperature profile - these are not observed changes at the surface (I.e. the record length is not 60 to 80 years). [Sharon Smith (Reviewer's comment ID #: 244-26)]	Noted. Text modified.
4-343	A	29:54	29:54	"currently" and "in the past 60 to 80 years" seems contradictory. [Chris Folland (Reviewer's comment ID #: 71-136)]	Noted and modified.
4-344	A	29:56	29:56	"From 1999 to 2004..." should be corrected to: "From 1999 to 2006…" [Ketil Isaksen (Reviewer's comment ID #: 115-5)]	Accepted. Text modified.
4-345	A	29:56	29:56	"Strongly" is probably not required - subjective. [Sharon Smith (Reviewer's comment ID #: 244-27)]	Accepted. Text modified.
4-346	A	29:57	30:3	Note that other tundra and high Arctic sites would be considered "powerful" indicators of climate change. An example is the site at Alert (see Smith et al. 2003 - citation provided in chapter ref list) in the Canadian high arctic - a similar description could have been provided for it and it also has a much longer record than the Norwegian sites. One of the reasons the change in permafrost temperature in the northern Mackenzie Valley is greater than the central valley is the reduced buffer layer. It would be more correct to say that there is a more direct link between air and ground temperatures at these sites (where snow is blown) rather than say they are a more powerful indicator of climate change. Geothermal regimes at sites that have significant snow cover to some extent are just as powerful an indicator of climate change - they however will also reflect changes in snow cover (another component of climate) in addition to air temperature. The statement given assumes that air temperature is the only component of climate that changes and is reflected in the record of ground temperatures. [Sharon Smith (Reviewer's comment ID #: 244-29)]	Noted.
4-347	A	30:0		Table 4.7.1., under section "Canada", last column: "Smith S. et al., 2005" should be corrected to: "Smith et al., 2005b" (four times, c.f. #3) [Ketil Isaksen (Reviewer's comment ID #: 115-6)]	Accepted. Text modified.
4-348	A	30:0		Table 4.7.1., under section "Europe", second column (Depth): Values should be corrected from "~5" to "~3" for Juvvasshoe, and from "~5" to "~2" for Janssonhaugen.	Accepted. Text modified.

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				[Ketil Isaksen (Reviewer's comment ID #: 115-7)]	
4-349	A	30:0		Table 4.7.1., under section "Europe", fourth column (Permafrost Temperature Change): Please replace the word "to" to "-" --> e.g. "0.5-1.0" and "1-2". [Ketil Isaksen (Reviewer's comment ID #: 115-8)]	Accepted. Text modified.
4-350	A	30:0		Table 4.7.1., under section "Europe", fifth column (Reference): Please delete "Isaksen et al., 2000" and replace with "Isaksen et al., 2001" --> Full reference (please add to the reference list): "Isaksen, K., P. Holmlund, J.L. Sollid, and C. Harris, 2001: Three deep alpine-permafrost boreholes in Svalbard and Scandinavia. Permafrost and Periglacial Processes 12: 13-25.". The reason for this replacement is that the site "Juvvasshoe" is not presented in "Isaksen et al., 2000", but in "Isaksen et al., 2001". [Ketil Isaksen (Reviewer's comment ID #: 115-9)]	Accepted. Text modified.
4-351	A	30:0		Table 4.7.1 - This table is an updated version of the one given in Romanovsky et al (2002) which was also included in ACIA. The caption should indicate that this is updated from Romanovsky et al. (2002) (Romanovsky, V., Burgess, M., Smith, S., Yoshikawa, K., and Brown, J. 2002. Permafrost temperature records: indicators of climate change; EOS, Transactions of the American Geophysical Union, v. 83 no. 50, p. 589) [Sharon Smith (Reviewer's comment ID #: 244-32)]	Accepted. Text modified.
4-352	A	30:0		Table 4.7.2. Note that the change in temperature that is given for Alert is for the shallower depths (15 m). At 30 m depth the change in temperature over the same period is much lower (0.06°C/year which gives a total increase of 0.3°C over the 5 year period) year. Note also that the total increase in temperature at a depth of 15 m between 1995 and 2000 based on 0.15°C/yr is about 0.75°C for the 5 year period (Smith et al. 2003). [Sharon Smith (Reviewer's comment ID #: 244-33)]	Accepted. Text modified.
4-353	A	30:0		Table 4.7.1. The depth of temperature measurements in the northern Macknezie valley is 20 to 30 m. Trends were examined between 1990 and 2002. The actual increase in temperature from 1990 to 2002 is between 0.3 and 0.8 °C (Smith et al. 2005). [Sharon Smith (Reviewer's comment ID #: 244-34)]	Accepted. Text modified.
4-354	A	30:0		Table 4.7.1. The reference for the southern Yukon Territory is Burn and Haeberli 2002 [Sharon Smith (Reviewer's comment ID #: 244-35)]	Accepted. Text modified.
4-355	A	30:0		Table 4.7.1. The reference for Northern Quebec 1996-2001 is Brown et al. 2000 (citation is inluded in the chapter 4 ref. list). [Sharon Smith (Reviewer's comment ID #: 244-36)]	Accepted. Text modified.
4-356	A	30:1		Section 4.7.2.3 A recent reference regarding Canadian permafrost region that could be added to this section is Beilman and Robinson 2003 (ref: Beilman, D.W. and Robinson, S.D. 2003 Peatland permafrost thaw and landform type along a climate gradient. Permafrost - Proceedings of 8th Int. Conf. on Permafrost. M. Phillips, S.M. Springman and L. Arenson ed. p. 61-65). The paper examines changes in peatlands and their	Noted.

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				vegetation related to permafrost degradation. Beilman and Robinson (2003) present some preliminary results which show that 10 to 50% of original frozen peat plateaus have degraded (with associated changes in vegetation cover) over the last 50 years in the discontinuous permafrost zone in western Canada. Recent results that show permafrost degradation over the last 50 years in the Mackenzie Valley are also presented. Climate is the dominant trigger for the sites studied. Another recent paper by Beilman et al 2001 (ref. Beilman, D.W., Vitt, D.H. and Halsey, L.A. 2001. Localized permafrost peatlands in western Canada: definition, distributions, and degradation. Arctic, Antarctic, and Alpine Research, v. 33, p. 70-77) could also be consulted for further evidence of permafrost degradation in western Canada. [Sharon Smith (Reviewer's comment ID #: 244-37)]	
4-357	A	30:2		Replace "powerful" with "more direct" [Govt. of United States of America (Reviewer's comment ID #: 2023-289)]	Accepted. Text modified.
4-358	A	30:3	30:3	Indicate that Murtel-Corvatsch borehole is in Switzerland [Sharon Smith (Reviewer's comment ID #: 244-28)]	Accepted. Text modified.
4-359	A	30:3		Add after Murtel-Corvatsch "in the Swiss Alps" [Govt. of United States of America (Reviewer's comment ID #: 2023-290)]	Accepted.. text modified.
4-360	A	30:12	30:16	There is some repetition here - repeating information summarized in first sentence? [Sharon Smith (Reviewer's comment ID #: 244-30)]	Noted and modified.
4-361	A	30:16	30:18	This section focusses on changes in permafrost not on changes in temperature of seasonally frozen soil. Perhaps this last sentence should be removed and included in the section on seasonally frozen ground. [Sharon Smith (Reviewer's comment ID #: 244-31)]	Accepted. Text modified.
4-362	A	30:20	30:22	Table 4.7.1 should be at least selectively cross referred to maps of temperature trends in CH3. Is there consistency? [Chris Folland (Reviewer's comment ID #: 71-137)]	Noted.
4-363	A	30:20		This table should be cross-referenced to Romanovsky et al 2002 and the updated version on page 211 of the ACIA report. The non-permafrost reader will be unaware that long-term changes are based on an extrapolated curve to the top of the permafrost table, thus the approximate 1 meter depth (base of active layer) for Lachenbruch and Marshall. Author should try to make this distinction otherwise the temperature changes are like comparing apples and oranges. Furthermore the period of record is not 1910s as there were no observations at that time in Northern Alaska; table is incorrect. [Govt. of United States of America (Reviewer's comment ID #: 2023-291)]	Accepted. Text modified.
4-364	A	30:20		Northern Quebec entry for 1996-2001 cites a 1995 reference, Change to Brown et al 2002 or Smith. 2005. Russia it's 2001 not 1002. [Govt. of United States of America (Reviewer's comment ID #: 2023-292)]	Accepted. Text modified.

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4-365	A	31:4	:47	This section would benefit from more integration with Chapter 6 of the ACIA report (pages 209-220). A key recent reference on degradation is the paper by Jorgenson on the Tanana Flats. Lines 43 -45: speculative sentence, recommend deletion, not related to observations. [Govt. of United States of America (Reviewer's comment ID #: 2023-293)]	Noted. Text modified.
4-366	A	31:9	31:9	Give a more exact location for this highway. [Chris Folland (Reviewer's comment ID #: 71-138)]	Accepted. Text modified.
4-367	A	31:10	31:10	Explain what a talik is. [Chris Folland (Reviewer's comment ID #: 71-139)]	Noted and see glossary.
4-368	A	31:11	31:13	This sentence should be replaced with the following statement "Overall, the northern limit of permafrost retreated about 0.5 to 1.0 km southwards and the southern limit moved northward about 1.0 to 2.0 km along the Qinghai-Xizang highway (Wu and Liu, 2003; Wang and Zhao, 1997). " [Govt. of China (Reviewer's comment ID #: 2006-47)]	Accepted. Text modified.
4-369	A	31:15	20:31	"The statements describing the amount of basal thawing of permafrost should include the depth of the permafrost base, ie. are these statements related to thick permafrost on the order of 100s of metres thick or thin permafrost a few metres to a few 10s of metres thick. This is important because considerable time is required for temperature changes at the ground surface to be realized at greater depths and therefore reflect changes in climate that may have occurred over a century ago in the case of thicker permafrost." [Govt. of Canada (Reviewer's comment ID #: 2004-148)]	Accepted. Text modified.
4-370	A	31:15	31:20	The statements describing the amount of basal thawing of permafrost should include the depth of the permafrost base, ie. are these statements related to thick permafrost on the order of 100s of metres thick or thin permafrost a few metres to a few 10s of metres thick. This is important because considerable time is required for temperature changes at the ground surface to be realized at greater depths and therefore reflect changes in climate that may have occurred over a century ago in the case of thicker permafrost. [Sharon Smith (Reviewer's comment ID #: 244-38)]	Accepted. Text modified.
4-371	A	31:17	31:19	The Technical Summary (TS page 23, line 26) cites the values in the Ch. 4 Executive Summary (Ch. 4, page 2, line 53) about permafrost thawing rates: "The permafrost base is thawing at a rate ranging from 0.02 m/year in Alaska to 0.4 m/year on the Tibetan Plateau." When I try to find the source of this statement in Ch. 4, what I find on page 4-31 (lines 17-19) is the following: "At Gulkana, Alaska, basal thawing of permafrost is at an average rate of 0.04 m per year since 1992 (Osterkamp, 2003). Over the Tibetan Plateau, the basal thawing rate of about	Noted. Text modified.

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				0.01 to 0.02 m per year was observed since the 1960s (Zhao et al., 2003)." Does the Chapter 4 text support the values (values of 0.02 m/year for Alaska and 0.4 m/year for the Tibetan Plateau) in its Executive Summary and in the TS? If so, where? [Melinda Marquis (Reviewer's comment ID #: 162-26)]	
4-372	A	31:18	31:18	Explain basal thawing. [Chris Folland (Reviewer's comment ID #: 71-140)]	Accepted. Text modified.
4-373	A	31:20	31:20	"When" should be "if" [Chris Folland (Reviewer's comment ID #: 71-141)]	Accepted. Text modified.
4-374	A	31:20	31:20	Replace "when" with "if"? I think a hypothesis is implied here. [Francis Zwiers (Reviewer's comment ID #: 305-23)]	Accepted. Text modified.
4-375	A	31:22	31:25	The first two sentences of this paragraph simply repeat information given as background on page 4-28. [Adrian Simmons (Reviewer's comment ID #: 242-84)]	Noted and modified.
4-376	A	31:22	31:37	Much of this paragraph discusses impacts of permafrost thaw and should be covered in WG2. This section could be made considerably shorter and would only include information that presents evidence of permafrost degradation. [Sharon Smith (Reviewer's comment ID #: 244-39)]	Noted.
4-377	A	31:24	31:24	"particularly" is meant. [Chris Folland (Reviewer's comment ID #: 71-142)]	Accepted.
4-378	A	31:24	31:24	Replace "particular" with "particularly". [Francis Zwiers (Reviewer's comment ID #: 305-24)]	Accepted.
4-379	A	31:28	31:30	The sentence is not relevant to WG1. [Roxana Bojariu (Reviewer's comment ID #: 24-7)]	Accepted.
4-380	A	31:29	31:29	Arctic biota (with a capital). [Francis Zwiers (Reviewer's comment ID #: 305-25)]	Accepted.
4-381	A	31:36	31:36	References of "Smith" are cited wrong. The two different authors "Smith", which have publications in the same year, should be distinguished by "a" and "b" after the year. For this "Smith" the publication should be cited: (Smith et al., 2005a). [Ketil Isaksen (Reviewer's comment ID #: 115-10)]	Accepted.
4-382	A	31:39	31:47	Is the erosion necessary linked to climate? [Sharon Smith (Reviewer's comment ID #: 244-40)]	Accepted and text modified.
4-383	A	31:40	31:40	Explain thermo-abrasion. [Chris Folland (Reviewer's comment ID #: 71-143)]	Accepted and text modified.
4-384	A	31:45	31:45	Replace "Lowering in permafrost stability" with "Decreased permafrost stability" [Frederick Nelson (Reviewer's comment ID #: 188-3)]	Accepted.

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4-385	A	31:49		Section 4.7.2.4. This section does not really discuss subsea permafrost in the context of an indicator of climate change. It also tends to focus on the impact of warming of subsea sediments (more a subject for WG2). While the section mentions that the thermal regime of subsea permafrost is primarility controlled by seawater temperature it does not present any information on changes in the subsea permafrost thermal regime that may be linked to recent changes in climate. The comments on gas hydrates also seems a bit out of place and again is very general in nature. The authors should consider removing this section. (note - similar section not included in FOD) [Sharon Smith (Reviewer's comment ID #: 244-42)]	Accepted. The section will be removed.
4-386	A	31:52	31:53	Regarding the statement that subsea permafrost formed as a result of inundation - This permafrost formed during periods of lower sea level when these areas were exposed to the colder glacial climate. It became "subsea" as sea level rose (note also warming and degrading over time as it is now exposed to warmer sea bottom temperatures) [Sharon Smith (Reviewer's comment ID #: 244-41)]	Accepted.
4-387	A	31:52	:53	Subsea permafrost did not form as a result of inundation. It formed when continental shelf was exposed to colder climates at low stands in sea level. [Govt. of United States of America (Reviewer's comment ID #: 2023-294)]	Accepted.
4-388	A	31:53	31:57	I am not sure I understand the reason for breaking the contributions (crustal, geoid) up into two parts here. Is this an attempt to discuss both tide gauge and altimetry based estimates of sea level (with the later being the "sea level" referred to in the second sentence?) If so, tide gauge estimates are also effected by geoid and ocean basin changes, and the first sentence should be changed to reflect this. In addition, it would be good to indicate that you are talking about two different types of sea level measurements. Note that ocean basin changes effect more than just the regional values, as there is a global bias introduced (p.25 42-45). [Mark Tamisiea (Reviewer's comment ID #: 262-4)]	Does not apply to Chaper 4 but to Chapter 5. Ch. 5 Response: The GIA effects are complex and interrelated. It is not intended here to provide a complete description, but we intend to show its impact on sea level explicitly and for non experts. We think the explanation is not incorrect and will standby the statements.
4-389	A	31:53	31:57	How were the references for this section chosen? Was this an attempt to find a general reference to take care of all of GIA? At the very minimum, an "e.g.," should be added before Peltier, 2001. [Mark Tamisiea (Reviewer's comment ID #: 262-6)]	Does not apply to Chaper 4 but to Chapter 5. Ch. 5 Response: Accepted, "e.g., " added before reference.
4-390	A	31:53	31:57	It is tough to generalize these numbers. The original submission had a reference (admittedly old) that reasonable variations in earth model could introduce a range of values of 0.5 mm/yr to averages derived from far field tide-gauges. [Mark Tamisiea (Reviewer's comment ID #: 262-8)]	Does not apply to Chaper 4 but to Chapter 5. Ch. 5 Response: The numbers provide an order of magnitude of these effects and their range. We think it's more

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					informative to give a range than an average value.
4-391	A	31:56	31:57	In this context, Plag, 2006, is an inappropriate reference. This study does not calculate these fields, nor does it use only the geoid and ocean basin contribution (this quantity is not discusses in the paper.) The study predictions from of RSL from Mitrovica. [Mark Tamisiea (Reviewer's comment ID #: 262-5)]	Does not apply to Chaper 4 but to Chapter 5. Ch. 5 Response: Reference to Plag (2006) has been deleted.
4-392	A	32:1	32:4	Taken in the current context, the impact listed is too low. First, the "... associated regional sea level variations reach up to a few 0.1 mm/yr" refers to departure from the globally-averaged background value. Second, this number would only be accurate in the far field of the ice sheet. In the near field, which is included in "regional sea level variations", the effect can be over 1 cm/yr (see Figure 3, Tamisiea et al., EPSL, 213, 477-485, 2003.) This could be important in some locations, such as Alaska. In fact, perhaps it would be a good idea to add the range of impact in both the far and near fields. [Mark Tamisiea (Reviewer's comment ID #: 262-7)]	Does not apply to Chaper 4 but to Chapter 5. Ch. 5 Response: We acknowledge the detailed comment of the Reviewer who is an expert in the GIA topic. However, we feel an elaboration of the differences between near fields and far field effects of the GIA would be too lengthy and cause an imbalance with respect to other sea level related phenomena. GIA influence on sea level is the main subject of this section, not the complete theory of GIA. As such, we choose to primarily describe GIA in terms of its effect as corrections to sea level measurements.
4-393	A	32:3	32:3	The gas hydrates need cross referral to other chapters, particularly CH7, [Chris Folland (Reviewer's comment ID #: 71-144)]	Accepted.
4-394	A	32:3	32:3	Is anything known about the status of subsea permafrost? [Francis Zwiers (Reviewer's comment ID #: 305-26)]	Noticed. Text modified.
4-395	A	32:9	32:9	Explain pedogenic. [Chris Folland (Reviewer's comment ID #: 71-145)]	Accepted. Text modified.
4-396	A	32:14	32:14	I think by "an artifact" is meant "a consequence"? [Chris Folland (Reviewer's comment ID #: 71-146)]	Text clarified
4-397	A	32:19	32:25	Looking at the figure, I think Russia is meant on line 22 as well. The whole report needs to have a consistent use of "Russia" and "Former Soviet Union". [Chris Folland (Reviewer's comment ID #: 71-147)]	Accepted. Text modified.
4-398	A	32:19	32:25	This paragraph seems out of place and the information in it regarding changes in active	Taken into account.

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				layer should go after the next paragraph that provides a description of the monitoring program. There also seems to be a lot of detail that is probably not needed. The reference given in this paragraph, Pavlov 1996 would appear to provide information that should have been included in TAR. Is there not more recent data that could be presented? Were there not recent papers that discuss Russian active layer trends in the 8th International Permafrost Conference Proceedings or in the special issue of Permafrost and Periglacial Processes edited by Nelson (2004 - ref given in chapter list). [Sharon Smith (Reviewer's comment ID #: 244-43)]	
4-399	A	32:19	:25	Difficult to follow which stations cited in text are being used for the results shown in Fig 4.7.1. Pavlov and Malkova have a 2005 publication that re-states many of these onclusions. Every effort should be made to update this Russia section based on the 2005 report. [Govt. of United States of America (Reviewer's comment ID #: 2023-295)]	Noted. Stations are plotted on the map and details are given in reference.
4-400	A	32:20		Early 1990s. the original 66 permafrost stations had been reduce to 25 [Govt. of United States of America (Reviewer's comment ID #: 2023-296)]	Noted. References are given for details and station information are not repeated here.
4-401	A	32:23	32:24	Figure 4.7.1 Caption states, "... in Russia from 1956 through 1990. Active layer thickness has increased about 21 cm ..." However, page 32, lines 23-24 state, "Over the period 1956–1990, the active layer exhibited a statistically significant deepening by about 20 cm." Should the figure caption be revised to cite "about 20 cm"? [Melinda Marquis (Reviewer's comment ID #: 162-25)]	Accepted.
4-402	A	32:24	32:25	Can this last sentence be more specific, "Changes in air temperature and snow depth…." - What are the changes, increase in air temperature etc. [Sharon Smith (Reviewer's comment ID #: 244-44)]	Accepted. Text modified.
4-403	A	32:29	32:31	The GTN-P is mentioned in this section (should define if not defined earlier) but should be mentioned much earlier as suggested in earlier comment on section on permafrost temperature. [Sharon Smith (Reviewer's comment ID #: 244-45)]	Rejected. This reort does not intend to endorse any national or international programs. That is why GTN-P is deleted.
4-404	A	32:29	32:42	This section should provide recent references instead of referring mainly to Brown et al.(2000) which will only include data up to 2001. There were a number of papers published in both the proceedings of the 8th International Permafrost Conference and also in special issue of Permafrost and Periglacial Processes edited by Nelson (2004 - ref given in chapter list). In addition Brown et al. (2000) only includes sites that are part of the CALM program and other relevant information on trends in active layer conditions should also be included. Some Canadian references to include: Nixon, M., Tarnocai, C. and Kutny, L. 2003. Long-term active layer monitoring: Mackenzie Valley, northwest Canada. Proceedings of the 8th International Conference on Permafrost, July 2003, Zurich	Accepted. But we cannot use all references mentioned here.

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				Switzerland. M. Phillips, S.M. Springman and L.U. Arenson (eds.), A.A. Balkema, Lisse, the Netherlands, p. 821-826. Tarnocai C., Nixon, F.M. and Kutny, L. 2004.Circumpolar-Active-Layer-Monitoring (CALM) sites in the Mackenzie Valley, Northwestern Canada. Permafrost and Periglacial Processes, vol 15, p. 141-153. Mackay, J.R. and Burn, C.R. 2002. The first 20 years (1978-79 to 1998-1999) of active-layer development, Illisarvik experimental drained lake site, western Arctic coast, Canada. Canadian Journal of Earth Sciences 39: 1657-1674. [Sharon Smith (Reviewer's comment ID #: 244-47)]	
4-405	A	32:29	:41	Spell out GTN-P. This section could be updated using papers published in special issue of Permafrost and Periglacial Processes (see Nelson 2005 [Govt. of United States of America (Reviewer's comment ID #: 2023-297)]	Accepted. Text modified.
4-406	A	32:32	32:35	This is repetitive - repeats information provided in opening paragraph of section 4.7.3.1 [Sharon Smith (Reviewer's comment ID #: 244-46)]	Accepted.
4-407	A	32:32	32:32	Should change sentence to include climate variability, ie. "…..permafrost to climate change and variability." [Sharon Smith (Reviewer's comment ID #: 244-48)]	Accepted. Text modified.
4-408	A	32:35	32:36	Note that 1996 was also a cold year in northwestern Canada and this is also reflected in thaw depth records. Reference for Canada that documents maximum thaw depth in 1998 etc. is: Smith, S.L., Burgess, M.M. and Nixon, F.M. 2001. Response of active-layer and permafrost temperatures to warming during 1998 in the Mackenzie Delta, Northwest Territories and at Canadian Forces Station Alert and Baker Lake, Nunavut; Geological Survey of Canada Current Research 2001-E5, 8 p. [Sharon Smith (Reviewer's comment ID #: 244-49)]	Rejected. No intent to discuss specific year.
4-409	A	32:38	32:40	Results from active layer thickness in Europe (Scandinavia) are not presented in Harris et al., 2003, but in Isaksen et al. 2006 (see complete reference in #4). In addition I suggest to add a sentence with results from Gruber et al. 2004 (already included in the reference list) and write the following: "Evidence from the Permafrost and Climate in Europe (PACE) program (Harris et al., 2003) indicates that active layer thickness in the mountains of Scandinavia has been the greatest in the hot summers of 2002 and 2003, approximately 20% greater than the previous years (Isaksen et al., 2006). During the unusually hot summer of 2003 in Central Europe, the thaw depth in the Alps probably exceeded previous maxima even on time scales of centuries (Gruber et al., 2004)." [Ketil Isaksen (Reviewer's comment ID #: 115-11)]	Accepted. Text modified.
4-410	A	32:46	32:48	This first sentence should perhaps be an introductory sentence to section 4.7.3 as this section (4.7.3.2) only referes to non-permafrost areas. Sentence should also be revised: "…whether it is underlain by permafrost"	Accepted. Text modified.

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				[Sharon Smith (Reviewer's comment ID #: 244-50)]	
4-411	A	32:46	33:8	The title of the section is "Seasonally frozen ground in non permafrost areas". Then the first sentence mentions seasonally frozen ground whether permafrost exists or not. Clarify what is being discussed in this section. Fig 4.7.3, referred to in this section, seems to include both. [Chris Folland (Reviewer's comment ID #: 71-148)]	Accepted. Text modified.
4-412	A	32:48	32:45	replace "underlain" with "underlying" [Frederick Nelson (Reviewer's comment ID #: 188-4)]	Accepted.
4-413	A	32:48		underlying instead of underlain [Govt. of United States of America (Reviewer's comment ID #: 2023-298)]	Accepted.
4-414	A	32:49	:51	Need to specify if these stations are in previously forested areas and were cleared and now covered by grass. Do they represent regional vegetation. [Govt. of United States of America (Reviewer's comment ID #: 2023-299)]	Noted. These stations are standard Russian hydrometeorological stations.
4-415	A	32:50	32:51	Increase in both winter air temperature and snow depth, or is one increasing and the other decreasing? [Francis Zwiers (Reviewer's comment ID #: 305-27)]	Noted. It indeed increases in both.
4-416	A	32:53		Are these soils on the Plateau underlain by permafrost. It's a permafrost region? [Govt. of United States of America (Reviewer's comment ID #: 2023-300)]	Noted. No, these areas in permafrost regions but not necssarily underlain by permafrost. Text modified to clarify.
4-417	A	33:4	:8	Rewrite first sentence; poorly phrased. Would be good to explain how these changes were computed; presumably not measured?? [Govt. of United States of America (Reviewer's comment ID #: 2023-301)]	Accepted.
4-418	A	33:12	33:20	Section 4.7.3.3 This result for Eurasia is rather surprising, i.e the forward advance of the freeze season in autumn. Is it consistent with temperatures and other evidence in CH3? and evidence from other chapters - like the NDVI vegetation index if that is discussed? [Chris Folland (Reviewer's comment ID #: 71-149)]	Noted.
4-419	A	33:15	33:15	Is this consistent, or inconsistent, with river and lake ice formation and break-up data in Eurasia? If inconsistent, why? [Francis Zwiers (Reviewer's comment ID #: 305-28)]	Noted.
4-420	A	33:22		Section 4.7.4. This section on consequences seems to be a discussion of impacts that would be more appropriate for Working Group 2. This section could probably be much shorter with a great deal of the material moved to the appropriate chapter (such as polar chapter 15) in WG2. There is also some repetition of material presented in section 4.7.2.3. If most of the material in section 4.7.4 was removed and discussed in WG2, there would be more space available to elaborate on observed trends in permafrost conditions etc. and also add more references.	Accepted. The section will be removed.

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				[Sharon Smith (Reviewer's comment ID #: 244-51)]	
4-421	A	33:22	:52	Presumably the thaw would also have some impact on CO2 uptake during the growing season. Why are consequences discussed in a chapter on observations? Shouldn't consequences of observations appear elsewhere in the Assessment? [Govt. of United States of America (Reviewer's comment ID #: 2023-302)]	Accepted. The section will be removed.
4-422	A	33:35	33:35	replace "melting" with "thawing" [Frederick Nelson (Reviewer's comment ID #: 188-5)]	Accepted.
4-423	A	33:36		why would these changes have resulted in increased runoff? One would have thought with more infiltration, to greater depth, at least surface runoff would have been reduced. [David Rind (Reviewer's comment ID #: 214-39)]	Noted. Text removed.
4-424	A	33:36		Why would these changes have resulted in increased runoff? One would have thought with more infiltration, to greater depth, at least surface runoff would have been reduced. [Govt. of United States of America (Reviewer's comment ID #: 2023-303)]	Noted. Text removed.
4-425	A	33:38	33:45	presumably the thaw would also have some impact on CO2 uptake during the growing season. [David Rind (Reviewer's comment ID #: 214-40)]	Noted.
4-426	A	33:39	33:40	Drainage will be an important factor. [Sharon Smith (Reviewer's comment ID #: 244-52)]	Noted.
4-427	A	33:47	33:48	Deepening of the active layer and associated effects on slope stability are not restricted to steep mountain terrain - this process can be important in any sloping terrain, river valleys etc. The sentence should also be rewritten: "…active layer may have an effect on slope instability..." as this will depend on such factors as ice-content, drainage etc. [Sharon Smith (Reviewer's comment ID #: 244-53)]	Accepted. The section will be removed.
4-428	A	33:53	34:41	Section 4.8 needs to cross refer significantly to CH5, and maybe have more cross refs earlier in the chapter as well than I have indicated. [Chris Folland (Reviewer's comment ID #: 71-150)]	Accepted. Text modified.
4-429	A	33:53		Section 4.8. There is very little discussion of changes in other climatic components other than air temperature. Climate change will involve changes in precipitation and particular snow cover. Linkages between cryospheric components are not discussed such as links between snow cover and permafrost. Changes in snow cover can counteract that of air temperature in terms of the changes to the permafrost thermal regime (see for eg. a recent paper by Taylor et al (2006): Taylor, A.E., Wang, K., Smith, S.L. and Burgess, M.M., Judge, A.S. 2006. Canadian Arctic Permafrost Observatories: detecting contemporary climate change through inversion of subsurface temperature time-series. Journal of Geophysical Research. 111, B02411, doi:10.1029/2004JB003208.) [Sharon Smith (Reviewer's comment ID #: 244-56)]	Rejected. The focus of this chapter is on observations of the cryosphere. Consistency across observations is discussed in Chapter 3.

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4-430	A	33:55	33:55	It is not clear what is meant by "ice declining" - referring to sea ice?, ice sheets? [Sharon Smith (Reviewer's comment ID #: 244-54)]	Accepted. Text modified.
4-431	A	34:0		Near Table 4.8.1, it would be valuable to add another table listing the potential sea-level change (total stored freshwater) that could result from total melting of each of the components listed in 4.8.1. [Govt. of United States of America (Reviewer's comment ID #: 2023-304)]	Rejected. This is given in Table 4.1.1.
4-432	A	34:0		Please discuss Chapter 10, which seems to be quite confident that Antarctica will have a negative contribution. That seems to contradict the positive Antarctic contribution that this chapter finds. Are there two schools of thought that each need to be reflected in both chapters? [Govt. of United States of America (Reviewer's comment ID #: 2023-305)]	Noted. Discrepancies have been discussed with Ch. 10, and appropriate changes have been made.
4-433	A	35:1		Section "References": Reference list is not consistent, e.g. at page 35, line 21 and 22: Publication year (2005) is placed at the end. [Ketil Isaksen (Reviewer's comment ID #: 115-12)]	Editorial corrections included.
4-434	A	36:1		Reference, add: Belchansky, G. I., D. C. Douglas, I. V. Alpatsky, and N. G. Platonov, 2004. Spatial and temporal multiyear sea ice distributions in the Arctic: a neural network analysis of SSM/I data, 1988– 2001, J. Geophys. Res., 109, C10017, doi:10.1029/2004JC002388. [Ola M. Johannessen (Reviewer's comment ID #: 119-15)]	Rejected.
4-435	A	36:1		Reference, add: Bjørgo, E., O. M. Johannessen and M. W. Miles, 1997. Analysis of merged SMMR-SSMI time series of Arctic and Antarctic sea ice. Geophys. Res. Lett., 24, 413–416. [Ola M. Johannessen (Reviewer's comment ID #: 119-16)]	Rejected.
4-436	A	36:1		Reference, add: Cavalieri, D. J., Gloersen, P., Parkinson, C. L., Comiso, J. C. and Zwally, H. J., 1997. Observed hemispheric assymetry in global sea ice changes. Science, 278, 1104-1106 [Ola M. Johannessen (Reviewer's comment ID #: 119-17)]	Rejected.
4-437	A	36:1		Reference, add: Deser, C., J. E. Walsh and M. S. Timlin, 2000. Arctic sea ice variability in the context of recent atmospheric trends. J. Clim. 13, 617–630. [Ola M. Johannessen (Reviewer's comment ID #: 119-18)]	Rejected.
4-438	A	36:1		Reference, add: Gloersen, P. and W. J. Campbell, 1991. Recent variations in Arctic and Antarctic sea-ice covers. Nature, 352, 33–36. [Ola M. Johannessen (Reviewer's comment ID #: 119-19)]	Rejected.
4-439	A	36:1		Reference, add: Johannessen, O. M., M. W. Miles and E. Bjørgo, 1995. The Arctic's shrinking sea ice, Nature 376, 126–127. [Ola M. Johannessen (Reviewer's comment ID #: 119-20)]	Rejected.

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4-440	A	36:1		Reference, add: Johannessen, O. M., E. V. Shalina and M. W. Miles, 1999. Satellite evidence for an arctic sea ice cover in transformation, Science 286, 1937–1939 [Ola M. Johannessen (Reviewer's comment ID #: 119-21)]	Accepted. Text modified.
4-441	A	36:1		Reference, add: Serreze, M. C., J. A. Maslanik, T. A. Scambos, F. Fetterer, J. Stroeve, K. Knowles, C. Fowler, S. Drobot, R. G. Barry and T. M. Haran. 2003. Record minimum sea ice cover in the Arctic Ocean for summer 2002. Geophys. Res. Lett., 30, 1110–111. [Ola M. Johannessen (Reviewer's comment ID #: 119-22)]	Rejected.
4-442	A	36:1		Reference, add: Stroeve, J. C., M. C. Serreze, F. Fetterer, T. Arbetter, W. Meier, J. Maslanik and K. Knowles, 2005. Tracking the Arctic's shrinking ice cover: another extreme September minimum in 2004. Geophysical Research Letters 32, L04501, doi:10.1029/2004GL02810. [Ola M. Johannessen (Reviewer's comment ID #: 119-23)]	Rejected.
4-443	A	38:49	38:51	Please delete reference "Isaksen K, Vonder Mühll…" (c.f. #9) and replace with: "Isaksen, K., P. Holmlund, J.L. Sollid, and C. Harris, 2001: Three deep alpine-permafrost boreholes in Svalbard and Scandinavia. Permafrost and Periglacial Processes 12: 13-25." [Ketil Isaksen (Reviewer's comment ID #: 115-13)]	Accepted.
4-444	A	38:49		Please add a new reference (c.f. #4): "Isaksen, K., J.L. Sollid, P. Holmlund, and C. Harris, 2006: Recent warming of mountain permafrost in Svalbard and Scandinavia. J. Geophys. Res., Submitted." (Current Stage: With Editor for Decision) [Ketil Isaksen (Reviewer's comment ID #: 115-14)]	Rejected.
4-445	A	44:36	44:37	Between line 36 and 37, add the following statement"Yao Tandong, Wang Youqing, Liu Shiyin, Pu Jianchen, Shen Yongping, Lu Anxin, 2004, Recent glacial retreat on water resources in Northwest China, Science in China (D), 47, 1065-1075." [Govt. of China (Reviewer's comment ID #: 2006-45)]	Rejected. Issue of WG 2.
4-446	A	45:19	45:19	Replace "in press" with "Vol 51, pages 509-527" [VINCENT GRAY (Reviewer's comment ID #: 88-539)]	Accepted.
4-447	A	46:0		The response to this question seems pretty technical for the anticipated audience for the FAQs. Also, I think it would be helpful to express all changes in the same way, if possible (i.e., either so much per decade, or total change since some date of departure). I think the latter is easier for non-technical readers to understand. [Francis Zwiers (Reviewer's comment ID #: 305-29)]	Accepted. Text modified.
4-448	A	46:7	46:7	Break this sentence into two sentences and insert a word the as follows: …with warming of the permafrost. Important coastal regions of the ice sheets ... [Wilmer Anderson (Reviewer's comment ID #: 5-34)]	Accepted.
4-449	A	46:9	46:9	FAQ 4.1: Need to add "ice caps" to statement. "The total contribution of glaciers, *ice caps* and ice sheets to sea level rise it estimated as 1.2 +/- 0.6 mm per year." This	Accepted. Text modified.

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				addition is necessary for consistency with Table 5.5.2 and with Table TS-3. [Melinda Marquis (Reviewer's comment ID #: 162-34)]	
4-450	A	46:10	46:10	FAQ 4.1: Need to add temporal qualifier "1993 to 2003." "The total contribution of glaciers, *ice caps* and ice sheets to sea level rise it estimated as 1.2 +/- 0.6 mm per year from 1993 to 2003." This addition is necessary for consistency with Table 5.5.2 and with Table TS-3. [Melinda Marquis (Reviewer's comment ID #: 162-35)]	Accepted. Text modified.
4-451	A	46:10	46:10	1.2 +/- 0.6 mm per year for what time period? Please add the time period. [Melinda Marquis (Reviewer's comment ID #: 162-72)]	Accepted. Text modified.
4-452	A	46:10		Suggest clarifying the use of sea level rise values. The answer text includes four such numbers qualified various ways. The value in the opening paragraph is not qualified with respect to time period whereas the other three are. The relationship between this opening paragraph value and the other three numbers is not clear. Seems like the sum value of 1.2 mm/yr should come directly from Table 5.5.2. [David Wratt & David Fahey (Reviewer's comment ID #: 67-35)]	Accepted. Text modified.
4-453	A	46:13	46:13	Should you add the qualifier " in April" after "snow cover"? [Melinda Marquis (Reviewer's comment ID #: 162-73)]	Accepted. Text modified.
4-454	A	46:15		Suggest changing to 'has occurred' for clarity. [David Wratt & David Fahey (Reviewer's comment ID #: 67-36)]	Accepted. Text modified.
4-455	A	46:24	46:24	Replace "2.7±0.7" by "2.7±1.4", to convert to 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-545)]	Rejected. Uncertainty limits are already 95%.
4-456	A	46:24	46:25	Perhaps consider rephrasing the statement about Antarctic sea-ice trend, e.g., something like "No statistically significant trend in Antarctic sea ice has been found." [Melinda Marquis (Reviewer's comment ID #: 162-74)]	Accepted. Text modified.
4-457	A	46:25	46:25	Replace "0.5±0.9" by "0.5±1.8", to convert to 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-546)]	Rejected. Uncertainty limits are already 95%.
4-458	A	46:26		Suggest for simplicity change to '…sea ice extent has declined by 7.4%…' [David Wratt & David Fahey (Reviewer's comment ID #: 67-37)]	Accepted. Text modified.
4-459	A	46:27	46:27	Replace "7.4±.9" by "7.4±5.8", to convert to 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-547)]	Rejected. Uncertainty limits are already 95%.
4-460	A	46:33	46:34	FAQ 4.1: Need to add "and ice caps" to statement. "Glacial *and ice cap* melt has contributed 0.51 +/- 0.32 mm per year to sea level rise between 1961 and 2003." This addition is necessary for consistency with Table 5.5.2 and with Table TS-3. [Melinda Marquis (Reviewer's comment ID #: 162-36)]	Accepted. Text modified
4-461	A	46:33		Suggest deleting 'at' [David Wratt & David Fahey (Reviewer's comment ID #: 67-38)]	Accepted.

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4-462	A	46:34	46:34	Replace "0.51±0.32" by "0.51±0.64", to convert to 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-548)]	Rejected. Uncertainty limits are already 95%.
4-463	A	46:34	46:36	FAQ 4.1: Need to add "from glaciers and ice caps" to statement. "Many northern hemisphere glaciers had a few years of near-balance around 1970, followed by enhanced shrinkage, with sea level contributions *from glaciers and ice caps* of 0.81 +/- 0.43 mm per year between 1993 and 2003." This addition is necessary for consistency with Table 5.5.2 and with Table TS-3. [Melinda Marquis (Reviewer's comment ID #: 162-37)]	Accepted. Text modified.
4-464	A	46:36	46:36	Replace "0.81±0.43" by "0.81±0.86", to convert to 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-549)]	Rejected. Uncertainty limits are already 95%.
4-465	A	46:38	46:38	Replace "sheets" by "sheets" [VINCENT GRAY (Reviewer's comment ID #: 88-540)]	Accepted.
4-466	A	46:38	46:38	Insert after "Greenland" "is growing" [VINCENT GRAY (Reviewer's comment ID #: 88-541)]	Rejected. Not supported by the assessment.
4-467	A	46:38	46:38	Replace "0.4±0.4" by "0.4±0.8", to convert to 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-550)]	Rejected. Uncertainty limits are already 95%.
4-468	A	46:38	46:39	FAQ 4.1: Consider citing contributions to SL rise separately for Greenland and for Antarctica, because the uncertainty for Antarctica's SL rise contribution is so great. [Melinda Marquis (Reviewer's comment ID #: 162-38)]	Accepted. Text modified.
4-469	A	46:38	46:39	Consider reporting separately the sea-level rise contributions of Greenland and Antarctica, so that at least one (Greenland's) is statistically significant. [Melinda Marquis (Reviewer's comment ID #: 162-75)]	Accepted. Text modified.
4-470	A	46:39	46:39	Replave "Antarctica are" by Antarctic ice is" [VINCENT GRAY (Reviewer's comment ID #: 88-542)]	Text is rewritten.
4-471	A	46:39	46:39	Replace "0.4±0.4" by "0.4±0.8"., to convert to 95 % confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-543)]	Rejected. Uncertainty limits are already 95%.
4-472	A	46:41		Suggest for clarity to change to '…increased ice flow velocity…' [David Wratt & David Fahey (Reviewer's comment ID #: 67-39)]	Text is rewritten.
4-473	A	46:47		Suggest for clarity to change to '… explanations of glacier changes..' [David Wratt & David Fahey (Reviewer's comment ID #: 67-40)]	Accepted. Text modified.
4-474	A	46:47		The more neutral word "sufficiently" might be better than "too" [Adrian Simmons (Reviewer's comment ID #: 242-85)]	Accepted. Text modified.
4-475	A	46:48		Suggest to remove redundancy by removing '...implicating increased local air temperatures, Similary,...' [David Wratt & David Fahey (Reviewer's comment ID #: 67-41)]	Text modified.
4-476	A	46:50	46:50	Insert after "that" "recent"	Rejected.

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				[VINCENT GRAY (Reviewer's comment ID #: 88-551)]	
4-477	A	46:51	46:53	I don't understand this statement about models. Why say "but without a significant trend in ice export"? *And* without a significant trend in ice export"? [Melinda Marquis (Reviewer's comment ID #: 162-39)]	Accepted. Text modified.
4-478	A	46:55	46:55	Is the word "environmental" necessary, i.e., in "local environmental warming"? If not, perhaps you want to delete it. [Melinda Marquis (Reviewer's comment ID #: 162-40)]	Accepted. Text modified.
4-479	A	46:56	46:56	Insert after "of' "recent" [VINCENT GRAY (Reviewer's comment ID #: 88-552)]	Rejected.
4-480	A	48:0	72:	All color figures must be readable by color-blind people. For example, Figure 4.6.1 is totally unreadable by a color-blind people. Be color-blind friendly please. This is a UN's document. [Kenichi Matsuoka (Reviewer's comment ID #: 172-45)]	Noted.
4-481	A	48:4		Add arrows to show mass/heat change between cryosphere and ocean/atmosphere. [Kenichi Matsuoka (Reviewer's comment ID #: 172-46)]	Rejected. Interaction is not the message of the figure.
4-482	A	48:4		time scale for "ice shelf/ice sheet margins" can be as short as years, not months (it's unnecessary to enhance such an unlike short-time scale variation here). [Kenichi Matsuoka (Reviewer's comment ID #: 172-47)]	Rejected. Larsen B break-up happened on these short time-scales.
4-483	A	49:1	49:7	Fig 4.2.1. Based on Table 4.2.1, a composite plot of March and April snow cover may increase the signal to noise ratio of the spring decline in NH snow covered area over this period. [Chris Folland (Reviewer's comment ID #: 71-151)]	ACCEPTED; figs revised to show March-April average
4-484	A	50:0		Figure 4.2.2 - what are the units of the change that is displayed? [Francis Zwiers (Reviewer's comment ID #: 305-30)]	REJECTED. The caption states that the units are in percent.
4-485	A	50:5	50:7	Fig.4.2.2. The meaning of red lines are not explained in the captions. [Roxana Bojariu (Reviewer's comment ID #: 24-8)]	ACCEPTED.
4-486	A	53:0		Figure 4.3.2 is hard to see, message is difficult to grasp. Graphic artist should redo. [Govt. of United States of America (Reviewer's comment ID #: 2023-306)]	Accepted -- figure has been redrafted in color for clarity.
4-487	A	54:6		I had to stop to think what a negative and positive date trend was. "negative trend" could be replaced by "trend towards earlier dates" and "positive trend" likewise by "trend towards later dates". [Adrian Simmons (Reviewer's comment ID #: 242-86)]	Accepted -- text modified.
4-488	A	55:0	56:	Figures 4.4.1 and 4.4.2 show the evaluation of only a single data set on sea ice extent and concentration and no error bars for the individual estimates for each year. There are a number of estimates of sea ice extent and concentration using essentially the same data sets by very different algorithms. These estimates can differ by more than 30%, especially	Noted -- local concentration estimates can differ substantially, but estimates of hemispheric ice extent based on different algorithms are very consistent

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				for new ice or melting ice, which are critical to these estimates. Please provide a true assessment of the variations in sea extent and concentration from using the published estimates for these values from various authors. These curves are sure to be quoted in discussions about the outcomes from AR4 and it is not fair to only provide the assessment of this change from only one estimate. [Govt. of United States of America (Reviewer's comment ID #: 2023-307)]	and all produce very similar estimates of trend (although typically based on slightly different time periods). The text has been modified slightly to note the consistency among estimates and to indicate that Figures 4.4.1 and 4.4.2 are presented as examples.
4-489	A	55:0		Figure 4.4.1 - the caption needs to say that these diagrams display anomalies (of annual means?) relative to some time period. [Francis Zwiers (Reviewer's comment ID #: 305-31)]	Noted – caption already states this
4-490	A	57:1	57:10	Fig 4.4.3. Given fig 4.4.2, it might be more interesting to plot on Fig 4.4.3 the long term sea ice changes from HadISST in March and September, also the seasons of generally max and min sea ice cover. [Chris Folland (Reviewer's comment ID #: 71-152)]	Accepted – Figure modified
4-491	A	58:0		Figure 4.4.4 - is the black curve also a model? If so, should another colour be used to give it less visual prominence? A bit more detail in the caption about the models would be helpful. [Francis Zwiers (Reviewer's comment ID #: 305-32)]	Noted – colors chosen to be consistent with original cited publication (to which caption refers reader for more detail)
4-492	A	59:5	59:7	Fig. 4.4.5 caption should have "sea ice" in front of "area flux" and in front of "volume". [Chris Folland (Reviewer's comment ID #: 71-153)]	Accepted – text modified
4-493	A	60:3		"Atlantic" means Scandinavian? Or does it include Arctic eastern Canada as well? Match this legend to the text (page 4-17 line 12). [Kenichi Matsuoka (Reviewer's comment ID #: 172-48)]	Accepted: details now given in Figure caption
4-494	A	60:6	60:6	Fig. 4.5.1 caption. Explain Stineman-smoothed. [Chris Folland (Reviewer's comment ID #: 71-154)]	Taken into account: Reference added
4-495	A	61:1	61:21	Fig. 4.5.2. The light red line is almost invisible. On line 11, CH3 uses the terminology "structural" for "methodological" based on a new paper referenced in that Chapter, so word this is preferable. On line 14, I think glacier area is meant. I found the nomenclature of all the curves like C1 and C2 etc confusing and the resasons for it not fully explained. Can it be simplified? The total uncertainty in glacier contribution to seea level change needs to be cross referred toCH5. Is this consistent with their conclusions? [Chris Folland (Reviewer's comment ID #: 71-155)]	Rejected: visibility of red line depends on printer quality. Accepted, "methodological" replaced by "structural" Accepted (line 14), text modified Accepted: nomenclature modified Rejected: Ch 5 takes values from Ch 4, not the other way round; consistence is updated.
4-496	A	61:6		double periods. Remove one period there.	Accepted: period removed

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				[Kenichi Matsuoka (Reviewer's comment ID #: 172-49)]	
4-497	A	62:1	62:7	Fig 4.5.3. If possible, can Scandinavia and the Alps be separated? - as the recent climatic controls on mass have been rather different. [Chris Folland (Reviewer's comment ID #: 71-156)]	Rejected: this would request for smaller regions in all continents (e.g. High Mountains of Asia). The different behaviour of Scandinavia and the Alps is mentioned in the text.
4-498	A	65:0	65:	This diagram should be redrawn with the more recent data of Zwally et al (2005) [VINCENT GRAY (Reviewer's comment ID #: 88-553)]	Accepted. Figure redrafted.
4-499	A	65:1	65:9	Fig 4.6.1. Add units to the inset graph. Figs 4.6.1 and 4.6.3 are nice additions to our knowledge and well presented. [Chris Folland (Reviewer's comment ID #: 71-157)]	Accepted. Figure redrafted.
4-500	A	65:9		give unit for M dot. [Kenichi Matsuoka (Reviewer's comment ID #: 172-50)]	Accepted. Figure redrafted.
4-501	A	66:0	66:	This diagram should be redrawn with the more recent data of Zwally et al (2005) [VINCENT GRAY (Reviewer's comment ID #: 88-554)]	Accepted. Figure redrafted.
4-502	A	66:0		Figure resolution is poor, and caption needs to stand alone and be clear. [Govt. of United States of America (Reviewer's comment ID #: 2023-308)]	Partially accepted. Figure resolution was a problem with transmission rather than with the original. Caption clarified.
4-503	A	66:1	66:11	Fig. 4.6.2. There is not enough discussion in the text of the discordant brown result showing overall Greenland mass balance increase. [Chris Folland (Reviewer's comment ID #: 71-158)]	Accepted, text clarified.
4-504	A	66:6		ATM stands for atmospheric model?? Spell it out. [Kenichi Matsuoka (Reviewer's comment ID #: 172-51)]	Accepted. Figure redrafted.
4-505	A	67:1	67:7	Fig. 4.7.2.The caption is inadequate. The colours for the continuous and discontinous permafrost are too similar. [Chris Folland (Reviewer's comment ID #: 71-159)]	Accepted. Colour-coding modified.
4-506	A	67:7	67:8	It might be helpful to add the word "downward" to "The blue curve has been shifted downward by 20 mm for clarity." [Mark Tamisiea (Reviewer's comment ID #: 262-9)]	Does not apply to Chaper 4 but to Chapter 5. Ch. 5 Response: Accepted, shift removed from figure.
4-507	A	68:0	70:	"Figs. 4.7.1, 4.7.2 and 4.7.3. It is unfortunate that the authors have only chosen figures related to seasonally frozen ground and active layer to accompany the section on frozen ground (section 4.7) which also includes permafrost. The first draft did include a figure which showed trends in permafrost temperature and it is unclear why this has been	The permafrost information is included in Table 4.7.1.

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				removed. Permafrost temperature (especially if below the depth of seasonal variation) is a much better indicator of climate change as the deeper temperatures filter out the shorter term variations in temperature that occur at the surface. Active layer thickness (or depth of frost) shows considerable inter annual variation and is not the best indicator for examining long-term trends. The authors should consider showing one figure that shows trends in permafrost temperature throughout the northern hemisphere (similar to fig. 4.7.2) - there are good published data for Alaska, Canada, and Europe which would facilitate this. The authors should also consider reducing the emphasis on active layer/seasonally frozen ground and perhaps only show one figure (combine fig 4.7.1 and 4.7.2 for example)" [Govt. of Canada (Reviewer's comment ID #: 2004-149)]	
4-508	A	68:0	70:	Figs. 4.7.1, 4.7.2 and 4.7.3. It is unfortunate that the authors have only chosen figures related to seasonally frozen ground and active layer to accompany the section on frozen ground (section 4.7) which also includes permafrost. The first draft did include a figure which showed trends in permafrost temperature and it is unclear why this has been removed. Permafrost temperature (especially if below the depth of seasonal variation) is a much better indicator of climate change as the deeper temperatures filter out the shorter term variations in temperature that occur at the surface. Active layer thickness (or depth of frost) shows considerable inter annual variation and is not the best indicator for examining long-term trends. The authors should consider showing one figure that shows trends in permafrost temperature throughout the northern hemisphere (similar to fig. 4.7.2) - there are good published data for Alaska, Canada, and Europe which would facilitate this. The authors should also consider reducing the emphasis on active layer/seasonally frozen ground and perhaps only show one figure (combine fig 4.7.1 and 4.7.2 for example). [Sharon Smith (Reviewer's comment ID #: 244-55)]	See 4-507.
4-509	A	68:0		Fig. 4.7.1. Active layer thickness as measured with standard methods is a strong function of subsidence due to melting of excess ice at the permafrost table (over masive ice, the active layer cannot thicken with warming surface temperatures but ground subsidence takes place). How was this effect accounted for? If no corrections were applied, a corresponding remark should be made in order to avoid misinterpretation. [Wilfried Haeberli (Reviewer's comment ID #: 94-12)]	Accepted. Text modified.
4-510	A	69:0		Please avoid unnecessary references to programs in figures as well as in text. There is no need to identify sites as part of the CALM program - just show the sites. We don't identify which stations are part of which network for other types of networks in the report and this chapter should do likewise.	Accepted. Text and figure modified.

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				[Susan Solomon (co-chair WG1) (Reviewer's comment ID #: 246-2)]	
4-511	A	70:5	70:8	Fig.4.7.3. What low pass filter is used? [Roxana Bojariu (Reviewer's comment ID #: 24-9)]	A 13-point filter (see Chapter 3) is used.
4-512	A	71:0		Figure 4.8.1 is very important summary figure, but it has no punch, no impact. Needs to be more attractive, eye-catching, as it will likely be grabbed and used by many speakers and educators. [Govt. of United States of America (Reviewer's comment ID #: 2023-309)]	Accepted. Figure modified.
4-513	A	71:0		Prefer use of permafrost in diagram (below word snow and in caption) since bullet refers to permafrost temperature. [Govt. of United States of America (Reviewer's comment ID #: 2023-310)]	Rejected. "Frozen ground" is used as the more general term.
4-514	A	71:2	71:2	Figure 4.8.1 states that "maximum extent of seasonally frozen ground has decreased by 7% in spring over the 20th century in the NH." This seems inconsistent with page 33 (lines 5-7), which state, "The maximum extent of seasonally frozen ground has decreased by about 7% in the Northern Hemisphere since the mid-20th century, while in spring, the decrease in areal extent ranges up to 15%." Shouldn't Figure 4.8.1 refer either to a decrease of 7% since the mid-20th century, or to a decrease of up to 15%? [Melinda Marquis (Reviewer's comment ID #: 162-33)]	Accepted. Figure modified.
4-515	A	72:1	72:10	Question 4.1, Fig 1. Suggest you adapt figs 3.2.7 from CH3 for the top and bottom graphs i.e. use the same data. [Chris Folland (Reviewer's comment ID #: 71-160)]	Noted. The data are the same.
4-516	A	72:5		Suggest improving the description of this figure for the non-expert reader. For example, the use of 'anomaly' needs to be explained in this context along with 'mass balance'. [David Wratt & David Fahey (Reviewer's comment ID #: 67-42)]	Accepted. Text modified.